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Obstetrics: The Science and the Art - Part III. The Therapeutics and Surgery of Midwifery; Chapter XIX. Childbed Fever

CHAPTER XIX.

CHILDBED FEVER.

THERE is a violent and dangerous disorder, usually known in the world as Childbed fever, to which all lying-in women are liable, which is often fatal, even in the sporadic forms; while the alarm created by the announcement of its outbreak is greatly increased whenever the malady happens to be prevailing as an epidemic. During epidemic prevalence of childbed fever, not only are those women who may have been recently delivered regarded as being in a perilous condition, but even those who are pregnant, and not yet advanced to the last stage of their gestation, are looked upon as persons placed in extremely critical circumstances.

It is not wonderful that such alarm should be excited among pregnant and parturient women and their friends, for it is well known that childbed fever destroys more women than all the other diseases and accidents of parturition put together. A physician cannot long practise Midwifery without discovering that a constant and wise vigilance is necessary to obviate the causes of such attacks, and cure the patient who has been unhappily seized with it. Scarcely any form of dangerous disorder is more insidious in its approach, or more rapid in its development when once its terrific train is set in motion; a development so rapid that the loss of a few hours, at the commencement, renders all after interposition fruitless and unavailing. Not a few of the victims perish within twelve hours, and some even within six hours after the first manifestation of the symptoms. This is one reason why an accoucheur, in full practice, can never feel entirely at ease with respect to women recently confined under his care; and happy for his patients, if his vigilance never sleeps; else there might be prepared for them a rapid and fearful destruction, whose first signal being unperceived, should baffle all his efforts and skill, too late applied.

If these observations are just, then the Student of Medicine ought to consider himself, in all honest conscientious views of duty, bound

to give to this subject a careful and even anxious attention; for it is a subject by no means fit to be studied without earnestness, nor cursorily. With it are connected many important principles, that seem of little value as to the matter in general, but which, when considered as relative to the causes, progress, and cure of our disorder, are of the highest importance. There are many good and scientific physiological physicians, who do not apply their general physiology to the elucidation of those mysterious appearances that are discovered in the course of a violent and fatal childbed fever. But the Student ought to be not only a scientific physiologist, but so practically familiar with the laws of life, and its phenomena, as to be able at once to understand and solve the curious riddles that are presented in childbed fever, in its various and complicated, or even in its simple forms.

I have said that there are curious riddles, or mysterious phenomena, in the disease now under consideration; and, I suppose, no one who shall go over the field of inquiry and observe the scandalous discrepancies and inconsistencies of our writers on the subject will cavil with me for saying so. Diseases that are clearly understood, and methods that are proved to be salutary and successful, unite all voices in the proposition as to their nature and cure; but, in our disorder, the utmost latitude seems to have been given to the imagination, so that a complete distraction of the professional mind appears, in the command of this one to regard it as a fever—of that one, as an inflammation—of another, as synocha or typhus; to bleed—not to dare to bleed—to salivate—to rely on opium—on ipecacuanha, on turpentine—on purgative drugs, on saline draughts; and so, of every possible suggestion of treatment, until the Student, confused and baffled in his groping after some sure foundation to rest on, gives up the search for truth in despair, and resolves to wait until the conflict arises, and to do as best he may. Yet all this confusion, disorder, and disagreement exists not in the disease, but only in physicians themselves: the disease is one, the writers and talkers are legion, each one having a thought of his own. Doubtless there is a truth of the matter, and what we want is that very truth; which, if it could be once mastered, would clear our art of this discreditable warfare of opinions upon a subject in medical practice, than which there cannot be one more interesting to us, whether as scholars merely, or as ministers of health unto many confiding friends.

The Student ought to resolve to make himself familiar with all these disputations, to the end that, at last, he should, if possible, come to some fixed and clear views of his own: but, in doing this let him first strip off every prejudice, and present his mind a *rasa tabula* on

which to receive the impressions of truth, and nothing else. In studying this subject, let him hear everybody speak, but let him afterwards form his own opinion and establish it on his own knowledge. And I now advise him to question and criticize every statement of mine, whether of argument or fact, and judge as of himself and for himself concerning their truth and value. Should he believe because Gordon believed, or act because Hulme thus acted, he would become Gordon's tool or Hulme's valet and slave. He would not be a scholar indeed, but only a pupil, and a very stupid pupil too; as seeing with his master's eyes, hearing with his master's ears, and aping instead of acting the master, the physician—who, if he be a true philosopher, is, as Hippocrates says, in that like a God. While I thus advise and even implore the Student to render himself master of this important topic, I don't forget that I also have very grave duties to perform as a writer on Medicine. I am not without some feelings of doubt and apprehension, lest, in the weighty matters of this discussion, I should unconsciously add to, rather than lessen the confusion and disorder that I have already complained of. I ought earnestly to wish that I may mislead no young man by persuading him in a false way of pathology or therapeutics as to this fatal childbed fever, nor set forth any opinions, or inculcate any method, which being adopted or pursued after my recommendation, should lead to disastrous results. I strive to liberate myself from the bonds of prejudice, and endeavor to take such a calm and earnest view of the case as may satisfy both my judgment and my conscience, and leave me without any self-reproach for wickedly joining a faction, instead of honestly adhering to the plain truth and the right. This I will endeavor to do; for I know that writers, even those who are as humble as I am, cannot print and make public their thoughts, and then recall or cancel what may be amiss or pernicious; and that a word spoken does not cease to live, but passes onwards and downwards like the generations of mankind, doing its good or its evil work by a traditional force, that appertains to all the words and works of men, whether wise or foolish, good or bad.

Having in a former edition of this work, and in other writings of mine, and particularly in my late work, on *Childbed Fever*, set forth the views with which my mind was early imbued by the precepts and the experience that fell in my way, and having observed my notions of the nature, signs, and treatment of our disorder to be criticized in many respectable Journals, I might be supposed more willing now than ever to solicit the favorable opinions of that class of the brethren who are occupied as Reviewers. I cannot, however, regard a medical Reviewer

as other than a medical Doctor; and since, as I have already stated, I find a really scandalous confusion in Medicine as to childbed fever, I can only look upon a Reviewer as *one* among the Doctors, and one who has no greater claim to shape my opinions on childbed fever than any other doctor of equal attainments. John Fernel, Felix Plater, or good old Monsieur Puzos, or Hippocrates himself, though long since dead, are still members of the ever-living Republic of Medical Letters, and are equal in my eyes to the moderns, as still my brethren and members of my calling. If I cannot change my opinions because those who died 300 or 2400 years ago thought differently, neither can I do so because cotemporary writers take views of the subject different from those to which, after many years of anxious study and reflection, I have arrived. It is but a few years since most of the continental physicians had not the least hesitation to believe that all these childbed fevers are the results of the material substance of milk, displaced from its true normal place in the mammary glands, and deposited, by a metastatic action, upon the womb and other parts that are affected in childbed fever; so that all obstructions, deposits, and swellings were considered to be what the writers denominated *dépôts laiteux*, or milk-deposits, otherwise called *lait repandu*, or milk dispersed throughout the body or the limbs. Vigarous, of Montpellier, an excellent writer, in maintaining, at p. 388, tom. ii., that a metastatic congestion of lymph and milk is the material cause of the disease, convinces himself of the truth of the proposition by *believing* that the purulent and other deposits found within the belly after death cannot be merely the product of the inflammation, because there is no "direct ratio of the product to the inflammation."

Puzos, who viewed the subject in the same light, cannot persuade me that the inflammatory effusions within the peritoneum are caused by metastasis of milk, notwithstanding he, under the falsest views of the pathogeny, was one of the best practitioners that ever exercised his art in the cure of them. He was *par excellence* a bleeder *coup sur coup*, as he calls it. Puzos, who died in 1753, still lives in his admirable writings. But though a teacher in our calling, I feel under no obligation to think and say yes, or no, because Puzos said yes or no. There be many authors, now dead, who command more of my respect and reverence than many that be now living; wherefore, I hold myself not bound by the orders of any one writer, or any school of writers. In the Republic of Letters, the majority cannot govern.

After the foregoing observations, I shall proceed to execute my purpose of showing to the Student my own thoughts as to childbed fever; and I must, in the very beginning, express the regret I feel, to

be obliged to use the denomination so commonly adopted for it, for I consider the word childbed fever to be a false and misleading word.

Words are signs of ideas; and many ideas are fashioned upon words, and so, are nothing more than a sign of a sign. When words truly represent ideas, they are faithful servants of the understanding, whose state and will they represent and obey; but when our ideas are fashioned or moulded upon words, those words often make false representations of facts to the understanding; and then, instead of being our faithful servants and ministers, they are traitors to us, and deceivers. Now the term childbed fever is one of these deceiving words; since it infuses into our understanding the false notion that our disease is a fever, whereas in truth it is not pyrexia, or fever, but a pure non-specific phlegmasia. Were it indeed one of the cardinal fevers of Stoll, whether inflammatory, pituitous, or bilious, it would be impossible to take such views of it as we must entertain, provided we regard it as a pure phlegmasia, whose pyrexial manifestations are the loyal and just expression of the degree and stage of the essential phlegmasia, and nothing more and nothing less.

It is therefore unfortunate for science that the word childbed fever was ever introduced, and I should at once proscribe it, if I could suppose myself of sufficient authority to enforce such proscription, which I am far from supposing. All that I can do is, to warn the Student to weigh well the meaning or value of the word, and at least in the beginning, to endeavor to liberate himself from the rising prejudice that the disorder is a fever because it is called childbed fever, for otherwise he will never treat it right.

The disorder here to be treated of is observed only in pregnant and lying-in women; yet it is not one, but many affections. It is inflammation of the womb alone; or it is inflammation of the veins of the womb; or it is inflammation of the peritoneum; or it is metro-phlebitis, or metro-peritonitis; or else a combination of metro-peritonitis with phlebitis. These are its several forms.

In the above enumeration of the several forms of our disorder, I have purposely omitted the cases of ovaritis and other topical lesions, such as inflammations of the Fallopian tubes, for I am convinced that we shall have a sufficiently clear view of the matter if we comprise all the puerperal cases in the single group, composed of metritis, metro-phlebitis, and peritonitis.

For a great many years past, the medical press has teemed with papers containing accounts of the signs left by our disease, and discoverable in the remains of those unhappy women whom it destroyed. It is everywhere conceded that these signs or appearances furnish

evidence of a foregone state of inflammation, during the fatal illness of the subject; and even Puzos and Vigarous, as well as their contemporaries, could not but see these signs, notwithstanding they were blinded by the prejudice of the general notion of metastasized milk or other fluids. Many hundreds, nay many thousands of bodies examined after death, have shown the womb covered with inflammatory exudation, and bathed with serum or sero-pus; or reduced to a state of ramollescence by the late inflammation. Similar appearances are seen as to the ovaries and tubes.

Multitudes of the victims disclose, upon dissection, no outward signs of disease of the uterus; but, upon laying it open with the bistoury, the whole uterus or a part of the inner wall is found totally softened and ulcerated, or gangrened. Again—the womb-substance may at the first aspect appear to be perfectly normal in volume, hue, and consistence; but, upon incising its texture, many of its veins and sinuses are observed to be distended with pus, which has been formed by the very vessels within which it is detected by the dissector. In other cases, the above appearances are discovered not only as to the uterus and its veins, but along with them traces of inflammation, with which the peritoneum, in whole or in part, had been affected during the several phases of the disease that caused the extinction of the woman's life.

Some of the subjects clearly show that the patient had suffered under an unmixed, uncomplicated inflammation of the peritoneum, which, though of such violence or extent as to end in death, did not at all involve the womb-substance, nor the veins of the womb.

In contemplating these three cardinal forms of childbed fever, I mean metritis, metro-phlebitis, and peritonitis, I say that the Student ought to conceive of them as having a primary seat either within the uterine veins, in the uterine substance, or on the intra-pelvic peritoneum.

Though the post-mortem traces of peritonitis are sometimes observed to have pervaded every part of that serous membrane, still it is reasonable to believe that the primary or incipient area or areas of the inflammation were directly connected with the womb, as constituting its serous lining, or else with the ovaries, or the tubes, or the round or broad ligaments, or vagina. Indeed, it is very little probable that the primary areas of the phlogosed serous membrane ever exist beyond the pelvis; and, if this opinion is well founded, it is a most important one, since upon it is based the indication that, provided the original area of inflammation is really located within the pelvis, we ought to be not only prompt in our design, but efficient in our duty to limit it

within that region, and constrain it at least not to advance beyond those bounds; and in impressing upon it, by means of our remedies, a tendency to recover by *resolution*. I have met with many cases of childbed fever, in their very early and forming stages, and I have always sought for and detected the signs within the pelvic region. When I have unhappily been called so late as to find the peritonitis had extended its area beyond and far above the plane of the superior strait, I have always had the most painful misgivings as to the impending crisis for the sick woman. A woman might well withstand a peritonitis, or even a metro-peritonitis, in which the peritonitic element of it should not migrate beyond the plane of the superior strait, whereas she must inevitably succumb under such an expansion of the area of phlogosis as might comprise within it the whole peritoneum, and, of consequence, all that it invests and can affect by its states of health or disease. No person will be bold enough, however, to deny that so violent an inflammation of the womb as might suffice to decompose and convert its substance into a soft, semi-pultaceous mass, must prove fatal to the patient; and I have seen a uterus that became completely softened by an inflammation of only two days' duration. Tonnelé has given abundant proofs that the cases may be as just stated. Yet it will be found that a majority of the fatal results arise from an extension of the areas of inflammation quite up into the belly, all whose viscera are to become involved in the destruction, inasmuch as their serous covering is a part and parcel of each one of them. It is this extension of the area that is to be chiefly deprecated and opposed by all the means of art.

If, as I above said, the woman must perish whenever the uterus is ruined by the inflammation, how much more surely must she be destroyed, if that inflammation, like a consuming fire, wrap every abdominal viscus in the blaze.

I cannot make an accurate computation of the number of superficial feet contained in the whole peritoneum, including all its duplicatures, but it probably might amount to some sixteen feet, all told. Let the Student imagine sixteen feet of such serous membrane in a state of inflammation, and then ponder for a moment upon the condition of the nervous mass of the woman. Under such distracting provocations, how could he suppose that anything like a steady and regular process of either general or special innervations could be maintained within the animal economy? And how can he doubt that all the organs, with their functions, would speedily fall into inextricable confusion and weakness, seeing that the dominant, conservative, directing powers of the nervous mass are deprived, distracted, baffled, and prevented, by

the conflicting impressions and perceptions that attend upon such vast disorders of the tissues?

But what I just now said is not only true, but most important, viz., that the serous coat of an organ is part and parcel with the organ. Hence, when the intestine, stomach, liver, spleen, &c., are, as to their serous portion, inflamed, there is an inflammation of the bowels, a gastritis, a hepatitis, a splenitis, &c., superadded to the metritis or the metro-phlebitis. This cannot be denied; and if not, then where is the ground for that astonishment that is often expressed on contemplating the early sinking state of the woman?

This very tendency to *sink* appears to me one of the strongest arguments that can be brought forward, by those who contend that we have here no typhus or true adynamic disorder to deal with, but only a diffused and deadly inflammation to combat. In typhus, the patient sinks, not because the organs have given way primarily, but because the cause of typhus produces a state of the nervous mass that makes it give way primarily, and the organs secondarily. Typhus depends not on acute meningitis, nor on any inflammatory or catarrhal accident, but it is a primary degeneration or altered crasis of the molecules of the nervous mass, caused by epidemical or contagious poisons. In our sinking childbed phlegmasias, on the contrary, the outburst is with the organs, and so to the subsequent overthrow of the nervous mass; the cases are at opposite poles. The nervous centres are the positive, the organs are the negative poles of the functions or powers of animals. Typhus rules at the positive, phlegmasia at the negative poles. As to any difficulty in accounting for the weakness or sinking in the terrible forms of childbed fever, I cannot conceive of it, after the foregoing statement and contrast. Indeed, the daily observed cases of sinking, or nervous shock that follow dreadful railroad accidents, wherein limbs are torn or crushed, may well convince any man that the nervous mass, at once and directly, succumbs, and fails under such shock; but it might quite as readily be supposed to fail under the perception of those multiplied and innumerable morbid impressions that are produced by many superficial feet of peritonitis affecting the most important organs, that are dying because their peritoneal portion is at the same time dying.

I have perhaps already said as much as is necessary to show my opinions of the nature or pathology of the disorder, as a mere general statement of it. I have not as yet spoken at large of the purulent infection of the blood, or pyæmia, that arises under certain stages of metro-phlebitis, in which myriads of pus corpuscles are evolved from the inflamed endangium. I leave these last matters to be treated of

in a future page, and now proceed to speak of the causes of childbed fever, reserving to myself the privilege of entering into fuller explanations of the pathogenic process and pathological states of our disorder.

A woman advanced towards the full period of utero-gestation, or one who has recently passed through the conflict of labor, ought to be considered, *ipso facto*, as in a state of precarious health, and peculiarly liable to be attacked with inflammation. The tissues most likely to suffer such attacks are those that have been directly concerned in the acts of gestation and parturition. In the course of a labor, there is always to be supposed such an amount of distension, pressure, distortion of parts, and check of the circulation and innervation, as to leave no room for surprise that inflammation ensues. Indeed, we have more occasion to be surprised at the failure than at the occurrence of phlegmasiæ as sequelæ of parturition.

In addition to these circumstances it ought not to be overlooked that the uterus, and the whole reproductive apparatus, indeed, are undergoing a rapid transition-stage—one in which they are to seek a state of organic rest or repose, which is only to be attained when they shall all have recovered their normal condition, as it exists in the non-gravid state of the woman. This very transition-state of the organs is one prompt to morbid deviation in its course, and inciting to attacks of phlegmasia.

Again: the blood of the woman during pregnancy, and in the lying-in is, for most individuals, highly charged with the fibrinous element; a state of the blood that is supposed to give a proneness to inflammatory seizure.

There is, also, in women recently confined, a marked diminution of the pressure which previously had acted upon all the parts contained within the abdomen. Such diminution of organic pressure acts in a way, somewhat like the lessening of atmospheric pressure upon a part, as in cupping, to allow the unsupported vessels to become over full. If a new-delivered woman should have the abdomen left without due support from the tonicity or muscular activity of the abdominal muscles, fasciæ, and skin, the circulation within could not but become more abundant; since the vessels of the mesentery and peritoneum must be fuller in relaxed than in well-condensed states of the abdominal integuments. Hence I say that relaxation of them, after labor, incites to inflammatory congestion.

Besides the above-mentioned incitements and provocations to inflammation, there often happen contusions, wounds, and abrasions, greater or less, of parts concerned in the labor; and inasmuch as many orifices

of vessels connected with the venous system of the uterus remain patulous long after the discharge of the secundines; and, further, as these orifices are bathed with corrupt and even putrid juices, collected within the womb, we are rather surprised to find the woman recover without accident, than to see her fall a victim to inflammation of the parts.

The placental disk of the womb, or that part of it which, during the pregnancy, was occupied by the after-birth, is left, by its separation in a quasi-traumatic state; so that, instead of readily recovering or healing, it may as readily, under peculiar circumstances, pass into a dangerous state of inflammation.

While enumerating the above circumstances as likely to incite to the attack, I am not forgetful of other so-called causes of childbed fever, nor among them, the Epidemic cause. There is scarcely any disorder that is more frequently observed to become epidemical than this: and it is a singular thing that it may become so for a single house, used as a lying-in hospital. Or, it may become epidemic in a town only, or in a certain district, a state or nation, or a continent. If this be true, then there is to be sought for, and, if possible, discovered, some c a u s e that can either exist as to a single house or even ward of a lying-in hospital, or as to a town, city, or whole country.

The medical writers who have furnished to the profession learned disquisitions on epidemic causation, have signally failed to point out to us what that cause is. And we are to this day as much in the dark on the subject as were the ancients at the date of Hippocrates' treatises on epidemical disorders.

Dr. Sydenham candidly acknowledges our ignorance of the material essence or epidemic cause of such diseases. And while he teaches us that there are what he signalizes as epidemic constitutions of the air, he by no means presumes to show further than the fact that one epidemic constitution is accompanied with a general prevalence of sthenic disorders, while another one is marked by its train of adynamic affections; and, further that epidemic causes act to produce frequent attacks of one set of anatomic tissues, to the exclusion of other sets. Wholly unable to find, in the meteorological states of the air, a clear rationale of these influences, he fell back upon the only remaining resource of the judgment, and showed how probable it is that telluric poisons, coming up from below the geological strata, may render the air unwholesome and inquinate.

My own opinion has long been, that many true epidemial diseases do depend upon causes that are produced beneath the earth's rocky crust; and that volatile and perhaps imponderable essences in which

they consist, may forever escape the extortions of the chemist, or the observations of the eudiometrist. The atmospheric ocean, at the bottom of which we live, to use the figure of Humboldt, cannot but have perpetual relations with the earth's more central and igneous mass. There may then be, and there are probably, many material essences or compounds, whose names are not yet, and perhaps never shall be placed upon the catalogue of simple substances. Such elements might inquinate the whole air of a place or a nation for months in succession, modifying the life-force of man in epidemic fevers, inflammation, &c., or they might even be supposed to rise like the material cause of cholera, in certain parallels and meridians, and even to make the circuit of the earth; as cholera has twice done, and as influenza has several times been known to do.

I believe it is correct to say that the meteorological documents have hitherto cast no real light upon the nature of epidemic causes; for those prevalent and wide-spread diseases that depend upon meteorological states are rather to be regarded as weatherly or climatic, than as true epidemial disorders. Hence, it appears to me that the word epidemic-disease, and the word epidemic-constitution, are declaratory and not explanatory words, and mean, indeed, little more than this, namely, that a disease is unusually prevalent, and that such unusual prevalence has some relation to a state of the atmosphere, of the precise nature of which all men are equally ignorant.

Now, although the Student may be unable to learn what it is that poisons the air, and makes us sick, he needs not to deny that the air is vitiated; for he cannot refuse to admit that the cause really exists, and that it must exist in the atmosphere, and not in the earth, or only in vegetable or animal substances upon the earth; inasmuch as if they were confined within the bowels of the earth, or unextricated from vegetable substances, or combined in the waters, they would prove innocuous. He can no more escape from the conviction of its existence than he can escape from the conviction that there is time, space, or force; of which, however, he can no more know what they are, than what epidemic-cause is. Inasmuch as he can reason upon time, space, and force, and make accurate computations of them though he knows them not; so can he, in like manner, reason upon epidemic-cause, and compute its date, duration, and violence, by its effects and results.

For example, let one inquire what it is within the animal economy that can be acted upon by such epidemic-causes. In the Hôtel-Dieu, at Paris, there have prevailed the most devastating epidemics of childbed fever, which, after disappearing for years, have recommenced their

ravages in the same establishment, again and again. The same is true of the Maison d'Accouchements or Maternity Hospital, of the Dublin Lying-in Hospital, of hospitals at Vienna, London, Berlin, Prague, Philadelphia, and, indeed, wherever there may have been established any large houses for the accommodation of numerous lying-in women. Such epidemic-cause has been in activity since the age of Hippocrates, twenty-three centuries ago. I made out from that elegant work of Dr. Meissner, *Die Frauenzimmerkrankheiten*, and from Ozanam's *Histoire Médicale Générale et Particulière des Maladies Epidémiques, Contagieuses, et Epizootiques*, a catalogue of dates and places in which epidemic childbed fever has prevailed from 1652 to 1845, with references to the authors who have mentioned or described them. I could easily have augmented this catalogue by additions from Churchill's account in the vol. of *Essays on Puerperal Fever*, republished by the Sydenham Society, were it not inconsistent with the objects and limits of this volume to do so. I shall, therefore, subjoin only the data found in Meissner and Ozanam, notifying that the authorities from Meissner are indicated by the letter M., and in Ozanam by the letter O.

TABLE OF YEARS AND PLACES OF EPIDEMIC CHILDBED FEVER, WITH NAMES OF AUTHORITIES.

A. D.			
1652.	Leipsic,	Welsch,	O.
1672.	Copenhagen,	Th. Bartholin,	O.
1723.	Frankfort and Leipsic,	F. Hoffmann,	O.
1746.	Paris,	De Jussieu and Col de Villars,	O.
1746.	London,	Moore, J. Clarke,	M.
1764.	Dublin,	J. Clarke,	M.
1765.	London and Derbyshire,	Moore, Butler,	M.
1767.	Normandy,	Lepecq de la Cloture,	O.
1769.	London,	Leake,	O.
1770.	Vienna,	Fauken,	O.
1774.	Paris,	Doulcet,	O.
1774.	Dublin,	Collins,	M.
1774.	London,	Moore,	M.
1776.	Vienna,	Stoll and Finke,	O.
1786.	Lombardy,	Cerri,	O.
1787.	Dublin,	Collins,	M.
1787.	London,	J. Clarke,	M.
1788.	Dublin,	Collins,	M.
1792.	Copenhagen,	Rink,	M.
1805.	Dublin,	Collins,	M.
1809.	Leeds,	Hey,	M.
1810.	Dublin,	Collins,	M.
1811.	Heidelberg,	Nægèle,	M.
1811.	Dublin,	Collins,	M.

A. D.			
1811.	Somersetshire,	Bradley,	M.
1812.	Dublin,	Collins,	M.
1813.	Durham and Northumberland,	Armstrong,	M.
1814.	Northumberland, Penn.,	S. Jackson,	M.
1815.	Munich,	F. Boyer,	M.
1818.	Wurtzburg,	Schloss,	M.
1818.	Northumberland, Penn.,	S. Jackson,	M.
1818.	Vienna, Dresden, Bamberg,	Kiwisch,	M.
1818.	Dublin,	Collins,	M.
1819.	Berlin,	Pfeuffer,	M.
1819.	Dublin,	Collins,	M.
1820.	Dublin,	Collins,	M.
1821.	Edinburgh,	Campbell and Mackintosh,	M.
1823.	Dublin,	Collins,	M.
1823.	Vienna,	Lippich,	M.
1825.	Hanover,	Dommes,	M.
1826.	Dublin,	Collins,	M.
1826.	Berlin,	Neumann,	M.
1827.	Barmen and Neuenhaus,	Sonderland,	M.
1828.	Dublin,	Collins,	M.
1829.	Paris, Dublin,	Tonnellé, Collins,	M.
1830.	Giessin,	Ritgen,	M.
1833.	Prague,	Quadrat,	M.
1834.	Paris,	Bouchat,	M.
1836 to 1843.	Vienna,	Klein,	M.
1838.	Stockholm, Paris,	Elliott, Voillemer,	M.
1839.	Wangen,	Zeugerle,	M.
1843.	Paris,	Bidault and Arnaut,	M.
1845.	Dublin,	McClintock,	M.

The foregoing catalogue might with ease be greatly amplified, were time and space allowed me here; but, even as it is, it may serve to show the Student that the *cause*, for there is a cause, may be in activity in many different and distant quarters, and in many consecutive years. At least, the inspection of the table ought to convince him that childbed fever is no rarely occurring disorder, therefore, is well worthy of his earnest contemplation.

The most singular known property of the cause of childbed fever is that it operates upon woman pregnant or lying-in, and upon them only; and that, while its reign causes terror and desolation among that class of persons, it never in the least degree influences the health or threatens the security of the virgin, the child, the youth, the man, or the married but non-pregnant woman. One may well feel amazed at such a proposition, seeing that pregnant and lying-in women, as to their generical nature, susceptibility, and forces, are like all other

women, and in a general sense participate in man's nature. How interesting then the inquiry as to what it is within the animal economy of the gravid woman, that can be acted upon by the epidemic-cause to pervert and develop the phenomena of the childbed fever only in that class of persons. And here, it seems to me, he may begin to perceive some ground to stand upon. What is it, then, within the animal economy of the pregnant or lying-in woman, that can be acted upon deleteriously by the poisoning force of the epidemic-cause, whereas all other members of the human race are not obnoxious to the power of that cause?

For my own part, I confess that when I contemplate the living being man, I am compelled to attribute to his nervous element, or nervous substance or mass, all his impressionable quality, as well as all his perceptive and motor force—not motion; for I can not but consider that he lives by his nervous mass, and through it alone; and that whatever, within him, participates in the condition of vitality, does so because, and solely because it participates in the nervous substance, nervous element or mass; and that nothing within him lives, save because of its nervous element. If the blood-disk lives, if the maculæ germinativæ are alive, if an ovarian ovule lives, if the nucleus of a cell is endowed with plastic and alterative forces, it is, in all these cases, because the disk, the maculæ or the nucleus are endowed with nervous mass, in the condition of what Oken calls point-substance. And now, having made this averment, it seems unnecessary to say that I consider the derm, muscle, mucous and serous tissue, the vascular, absorbent, and indeed all the tissues, whether of a general or a special anatomy, to be endowed with life-force, only in virtue of the nervous mass within them, without whose presence and combination in them no vital power or organization could possibly exist in them.

This is the ground upon which, I say, the Student can stand, and on which he shall be sure at last to find a firm and solid footing.

Seeing that neither meteorology, nor eudiometry, nor any power in chemistry, can ever furnish him with a rationale of epidemic-causation, let him, for the sake of argument, admit that the atmosphere may be rendered unwholesome or inquinate by telluric poisons mixed with its lower strata; then it is easy to conceive that these poisons, like opium and the narcotics; like arsenic and mercury; like diffusible stimulants, as ether, chloroform, and other such, may imperceptibly modify the crasis and healthy force of the nervous mass, to that degree as to extend the ravages of the Black death, of English sweat, Syrian plague, or Asiatic cholera, or Influenza, among thou-

sands or millions of our race; or prevail, as epizootic causes, over flocks and herds, and the beasts of the field, or fishes in rivers and in the ocean. It is easy also for the mind to discover that such an epidemic-cause, though it bring within its range men and boys, unmarried women and children, as well as pregnant and lying-in women, shall not be able to effect such considerable morbid changes in the nervous mass as to make any of these classes become victims to it, save pregnant and lying-in women alone; and these, only because their peculiar condition serves, as exciting cause, to bring out into full manifestation the power of the true proximate or epidemic-cause. Epidemic-causes or poisons, such as this, must necessarily be esteemed to be very weak as to their influence on human health; else, we should certainly observe changes in the health of other persons besides the pregnant and lying-in. Still, even such feeble poisons may be able to impress on the body a tendency to become diseased—and such tendencies once established, it is necessary to look no farther than to pregnancy and labor for the exciting and operative causes of the malady. A curious remark is to be found in Dr. Collins, p. 386, which shows that the modifications of the air that precede the eruption of epidemic childbed fever in hospitals are gradual. "Dr. Joseph Clarke states it was generally observed that, previous to puerperal fever becoming epidemic in the hospital, the patients recovered more slowly; or, to use the language of the nurses, it was much more difficult to get them out of bed than usual. This, from experience, I have no doubt is the case; and, when observed, should arouse the medical attendant to adopt, without delay, every means he considers in the least calculated to prevent its occurrence."

I am quite sure that all those practitioners who have lived during the prevalence of the epidemic, even without meeting with the cases in their own practice, must have made the same observation as to the slowness and difficulty of recovery in the majority of lying-in women during the reign of an epidemic childbed fever; and hence we may rightly infer that the *cause* has acted on these very persons that Dr. Clarke describes, and the like of which have been seen by almost all obstetricians, but so feebly, as only to render them unfit to "get out of bed" as soon after the confinement as is to be usually expected. For my part, I cannot doubt that, during the reign of epidemic cholera in Philadelphia, when we lost some seven hundred citizens out of 420,000 souls, there were more than 100,000 persons suffering more or less from the operation of the cause; whereas, in fact, not many thousands of them felt or perceived anything positively amiss in regard to their health. Those only in whom some occasional cause

came to start the train of morbid symptoms, could give evidence of the power of the proximate *causation*.

In like manner, I conceive that, in a fatal epidemic childbed fever in Hôtel Dieu or Dublin Hospital, the cause could not but embrace within its epidemic-sphere all the nurses and attendants in the house; but, wanting the occasional causes, none sicken save the pregnant and lying-in inmates of the houses, the districts, or states, comprised in the catalogue. I conceive that these remarks are just, and that they show how it may happen that an epidemical constitution of the air may incline many women in childbed to fall sick with peritonitis, or metritis, or phlebitis, although the *cause* is of such feeble power as to be wholly incapable of making any persons except these very women fall sick.

Many medical men, and along with them a major part of the unthinking, unreasoning public, looking in vain for rationales of the cause of childbed fever, endeavor to satisfy their hankering after knowledge on that point, by adopting the notion that the cause of *epidemic* childbed fever is a contagion.

The Student will naturally be desirous to learn, if childbed fever be really a contagious disease, what the principle of that contagion is; and I apprehend that here, as in the instance of the epidemic malady, he shall have to rest content with the *sound* of the word contagion, a word which, being interpreted, means communicable from person to person, or by individual to individual. This is the whole meaning of the word; for, as to how, and the what, no man hath yet obtained the least definite notion, since no man hath known or can know what a miasm or a contagion is. Miasm and contagion are words, nothing more; they represent no precise material idea of the mind.

The notion of a contagion of childbed fever, communicated from individual to individual, either immediately or mediately through a third person, has arisen upon the observation of what are asserted to be facts. It has often happened, for example, that a woman in a lying-in ward, dying with the disease, has been speedily followed to the grave by other women occupying the same apartment; and it has at once been assumed that the second and third victims had taken the contagion from the first victim, and that, without inquiring how it happened that the first patient acquired the disorder, or whether it was probable that the others might take it in the same way. Again, a physician is observed to meet with many cases in his own practice, while his brethren in the same town or district meet with none such at all: or, a monthly nurse, going from one sick woman to another woman who speedily falls sick with a like disorder, gives rise to an

impression that she or that the physician should be looked to as the conveyers of the poison or contagion. Such occurrences are not rare in the history of medical men and nurses; so that a physician or a nurse, in this predicament, has been called a walking pestilence.

There is a striking example of this tendency, of what I cannot but consider weak minded people to jump at conclusions without looking to see where they leap, in the celebrated case of the Manchester Epidemic, the history of which is given by Dr. Roberton. In regard to that epidemic, I do suppose, that at least half of the people who have heard of it, believe that the outbreak of the childbed fever there consisted in the thirty cases that happened to fall out in the practice of that unfortunate midwife that Dr. Roberton tells us of—and if it were true, that these were the only cases, one might be tempted to suspect that a contagious fever had something to do with the matter. Almost all the contagionists who seize on this epidemic as the positive proof of their correctness, have ignored the more important facts, that the disease prevailed in all classes, without exception of rank or station, and that hundreds died with it, and that even Dr. Roberton himself declares, that in many of them there could be no ground to suppose that a contagion had anything to do with the victims.

Deeply impressed as I am with the importance of this question, and quite aware that argument and proof are often alike incompetent to change the stubborn fixed opinions of men, I cannot, however, refrain from setting forth the reasons that compel me to dissent from the doctrine of contagion in childbed fever. I shall, therefore, speak my real opinions, notwithstanding I know they will be combated, and in many instances promptly rejected, and even with disdain. I feel the question to be a most important one, inasmuch as, if we are to accept the notion of a contagious origin, we ought also to meet the consequences of that dogma. Certainly, that man must be an unfeeling and wicked wretch, who, believing in the contagion of childbed fever, should yet continue to exercise his ministry at the risk of carrying death and desolation into whatever family he should be called on to act the part of the obstetrician. For, if one case is communicable, another must also be communicable; and such a believer is bound in honor and honesty to desist from, or suspend his ministry elsewhere, as soon as he happens to be called to any case. Let him not change his dress, and purify his person, and then go like a poisoner, carrying with him wherever he goes, a peripatetic doom. Nobody has told him it is his dress that poisons—the malady is contagious from person to person, and not from dress to dress. Let him stop then at once, nor visit another patient until a long and perfect quarantine shall have

made him no longer dangerous as the upas. If he should have another case, let him stop again in time. Certain writers do make a distinction betwixt sporadic cases as non-contagious, and epidemic cases as highly contagious. But, who is he can discriminate in a sporadic case that destroys, and an epidemic one that destroys in an exactly like manner? Whether the woman perish with sporadic or with epidemic childbed fever, the signs, seats, lesions, and results are precisely the same as to the victims, and the power of generating contagion must be ever an identical power. It is useless to cavil with me on the facts. Nothing is more false than what are called facts, since nothing is so difficult as to know what a fact is. The lawyers know this, and even their cross-examinations leave them in so great an ignorance, that the jury vote is always a matter of doubt—and even when given, often wrongfully given. I have carefully read the cases, considered the arguments, and witnessed many of the events upon which so confident a belief of the contagion is founded—and I aver that I do not discover in them any force, that ought to convince me of the contagious nature of the disorder; wherefore I utterly reject and deny the doctrine as one injurious to the profession of medicine, pernicious to the people, by filling the minds of interested parties with alarm, and as propagating, from age to age, a vile demoralizing superstition as to the nature and causes of many diseases.

Should the Student ask me how to explain the curious occurrence of cases in the practice of one medical gentleman, while his neighbor meets with no such cases, I cannot account for so great a mystery; one which evinces rather a strange coincidence of accidents, than a peripatetic causation by the doctor. I prefer to attribute them to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea, at least as to this particular malady. To show how such things do happen, I will here relate an incident, which at the time of the occurrence made a powerful impression upon my mind.

A medical gentleman of my acquaintance left a patient whom he had just delivered, and immediately crossed the street, where in a house opposite he delivered another woman very speedily. The lady first mentioned was seized with epidemic childbed fever, and died; the second one got well without the least accident. He was then in the height of an epidemic, which seemed to single him out from among all his brethren as alone doomed to meet with these dreadful cases, of which they had none in their practice. The question may be asked, how happened it that, if he conveyed the poison, he did not convey it to both these women? It is true the Student may reply he was poi-

sonous, but only one of the women was susceptible; and the proof that he was poisonous is contained in the fact that he lost sixteen women, whereas other men lost not one. If the Student replies after this manner, let me say that, within a few days after the above-named occurrences, he invited me to visit with him a woman, more than two miles distant from the before-mentioned dwellings, and whom we found moribund with childbed fever. He was greatly affected by thus finding his patient in the article of death, and as we left the door together, he said, "Am I not singularly unfortunate? Here is this poor woman now dying, while in yonder house, a little way off," pointing to it, "lies another patient of mine, who is likewise dying. What a singular coincidence of cases! That case, over yonder, is one of a woman whom I have repeatedly assisted in her labors. A good while ago she sent me a message to engage me to wait upon her about this time. Having heard nothing from her, and being anxious on account of this epidemic, I called at the house to-day, to learn if she was well. I was told that, having been suddenly seized with labor, and rapidly delivered, they had not been willing to trouble me to call. That she had continued quite well for a day or two, but now was very sick, and would be glad to see me. Upon approaching her bedside, I found her irrecoverably ill with childbed fever, and she is actually in a dying state."

Such was the statement of my medical friend; and I have related it in order that I might next ask the Student to see, in the circumstances of the case, the undeniable proof that strange, inexplicable coincidences may cause one medical gentleman to be tracked, as it were, by childbed fever cases, without any complicity of his person or clothing in the causation of them. In this instance, he had not seen the woman until he found her perishing; in the other, if he gave the malady to Mrs. A, and did not give it to Mrs. B, did he communicate it to Mrs. C? and, if he did, where did Mrs. D find it? I said he lost sixteen patients out of a great number. Did he give it to number one? No. Did number one enable him to transfer it to number two? No. Why should the Student believe (he cannot know) that he gave it to number three, since number four unquestionably did not receive it from him? Well, then, if number four did not receive it from him, why should the Student charge him with giving it to five, six, seven, eight, nine, and so on to the last of the melancholy list? For my part, I am sure he neither did nor could communicate it to anybody. If the Student still insists that he was the conveyer of the poison, how did he convey it? Had he himself the disease? No. Did he convey it in his clothes, or hair, or scarf-skin, or in his pulmo-

nary or cutaneous halitus—which of these? But another friend of mine who had been *chased*, so to speak, by a series of such cases, seventy in all, left the city, was absent many days, and on returning, shaved his head, got a new wig, new clothes, new gloves, new pencil. He went into a bath, was washed clean, dressed himself, and then visited and assisted a woman in labor who was seized *next* day and died. Prussic acid, or arsenic, or nicotine, would hardly be more poisonous than such a gentleman, if he *was* poisonous, which he was not; he was only unlucky in meeting with the epidemic cases. I say he was not poisonous; but, if the Student insists he was poisonous, pray, how was he so? Not in his hair, which was cut off; not in his dress, it was new; not in his health, for he was in good health; everything, except the man, was new. He could not have carried the atmosphere of his last patient's chamber with him to the country, keep it about him like an invisible cloud, during many days; and then, after a bath, carry it into the last patient's chamber to destroy her with exhalations more pestiferous than the breath of Cacus. I have just now, since writing the last sentence, read again the observations upon the contagion of childbed fever, by Dr. Robert Lee, beginning upon page 487 of his admirable *Lectures on the Theory and Practice of Midwifery*, 8vo. 1844. No author has given or can give a clearer or more candid detail than his of the motives to believe or reject the contagion, as a cause, as far as those motives are discoverable in the observed cases. The esteem and veneration I feel for that wise and good man, Dr. Lee, are so great, that, if I could believe because any living physician believes a dogma, I should believe it because he does. But Dr. Lee does not believe; he only suspects and fears. At page 489, he says of the facts, that, "though they have led me to adopt the opinion that the disease is sometimes communicable by contagion, and sometimes has a connection with erysipelas; they have not, perhaps, been sufficiently numerous, and of so decisive a character, as to dispel every doubt on the subject of its contagious or non-contagious nature, and prove that it is a specific inflammation. It is but right to state that, in a vast majority of cases, the disease has occurred, and in the most destructive form, where contagion could not possibly be supposed to have operated as the cause."

Dr. Lee gives a summary of opinions of past writers, whom I shall not cite for want of space in this volume.

I refer to a special work on this subject which I published in 1854, entitled *Treatise on Childbed Fever* for an elaborate statement of the argument, both for and against the contagion of childbed fever, and I am willing to leave the subject there, for I am persuaded

that if my exposition of the doctrine of this contagion, in that treatise, is insufficient to bring the reader over to my way of thinking, I at least can never convince him, and must be content forever to let him alone in his phantasy.

Let not the Student charge me, then, with ignorance of the facts relative to the so-called contagion of our dreaded disorder. I am by no means ignorant of them as they are stated by numerous authors; and having, moreover, conversed on the subject with many of the most eminent living accoucheurs, both at home and abroad, I am ignorant neither of facts nor opinions; I even wholly reject the contagion of scarlatina, measles, and pertussis, as I do that of yellow fever and cholera. And I shall here take the liberty to excuse myself to my brethren who have opposite views, beseeching them to observe that, while I contend for the faith I have, I do not condemn *them*, however I may dissent from their *opinions* on these points. Let them therefore, in like manner, condemn and confute my *opinions* on childbed fever contagion, and, if possible, pardon *me*.

A man living by the edge of a swamp or mill-pond, in September and October, is very likely to have an attack of intermittent fever. A marsh-miasm makes him sick, and that after a particular manner. But what is the marsh-miasm? No man hath answered that question; and yet, everybody agrees there is some unknown relation betwixt the swamp and the morbid phenomena observable in the man, and that unknown relation is expressed in the word miasm. In like manner, a woman delivered in a lying-in ward of the hospital, falls sick with childbed fever. She is the first on the list; but her case may be followed by ten or by two hundred other cases. As to the marsh-miasm, I know it not; I only know that it is; and, in like manner, I know not what the ward-miasm is. I do know, however, that it is. The marsh-miasm makes sick unto death. The ward-miasm makes also sick unto death. A hundred men shall fall sick in the Valteline, or in the Campagna, and a hundred women shall fall sick in the hospital. Which is a contagious cause, and which is not a contagious cause? The Student may say he suspects the ward-disease to be contagious, but let him not say that he knows it to be so; πιστις and γνωσις, belief and knowledge, are very different states of the human understanding.

Let him say, if he will, I know that Mrs. A communicated her epidemic childbed fever to Mrs. B, and I know it, because Mrs. A died on Wednesday in the same ward with Mrs. B, who took it and died on Saturday. If the Student should thus speak, I must reply to him that, as he admits it arose spontaneously in Mrs. A, he ought not to

assume that it could not spontaneously arise in Mrs. B. In such a contagious malady as smallpox, if A be sick with it, and B come into his house, and after some days of incubation should fall sick with variola, he has a reasonable inference from A to B, and that inference is confirmed and reduced from belief or probability to knowledge, because he knows he could have taken lymph or pus from A, with which to inoculate B; an inoculation that could scarcely fail to produce smallpox. But, in childbed fever, he can have no such reasonable inference, since he cannot inoculate B with the malady of A, and knows not where to look for any virus, and has not the least notion of what or where the cause is. In smallpox, though he cannot know what the cause is, yet he can and does know where to look for it, and he always finds it combined with or contained in the lymph or pus of a variolous pustule.

Inasmuch as we cannot know what the essence of a miasm or contagion is, we ought to examine ourselves—that is to say, we ought to pry into our understanding, and see, if possible, whether, under these mysterious circumstances, there is not discoverable some principle, some law or fact, whether in our own understanding, or in the nature of the patient, which, being duly comprehended, might lead us to absolute knowledge upon this subject, and so, free us from miserable doubts and misgivings of duty; and to this end, it seems to me that such a self-examination must result in this, namely, that a miasm or a contagion, can act only on the sensitive element of bodies, just as medicines act solely by the impression they make upon the same sensitive element. As medicines do not act upon the dead body, neither can miasms or contagions affect it. Miasms and contagions are in the same category of powers as medicines.

Every person who has paid some attention to what is called method, or classification in Natural History, knows that the whole of the zoological series have been arranged upon the principle of the subordination of the organs.

"Vertebrate animals possess a trunk, on the sides of which all their parts are symmetrically arranged, because their nervous system consists of a central medullary cone, from each side of which proceed, in symmetrical order, the nerves of all the parts. The body of a mollusk is a lump, because its nervous system is confused; the articulates possess a greater degree of symmetry, because their nervous system is, to a certain extent, symmetrical; their body is articulated exteriorly because the nervous system is so within: in fine, even in the radiated animals, the last vestiges of a nervous system, to be traced in them, have the same radiate form which their whole body has."

"The form of the nervous system, therefore, determines the form of the entire animal, and the reason why it is so is plain; it is because in fact, the nervous system constitutes the entire animal, all the other systems being added merely to serve and maintain it."—*Flourens.*

The above paragraphs, from p. 88 of M. Flourens' *Analyse Raisonnée des Travaux de Georges Cuvier*, are cited to confirm the proposition that the nervous mass is, in fact, the only part or element upon which medicines, miasms, or contagions can act; and that, if it be true that the animal is what it is, in consequence of the arrangement and distribution of the nervous mass within it, then we may discern, in states of the nervous mass, causes why the organs should become pathological seats, instead of continuing to be the seats of physiological action or force.

The nervous mass, then, is not only the elaborator, but the creator of its organs; maintaining their health and life, or, loosing the reins of government, allowing them to fall into disease or death.

I pray the Student, then, to look upon the nervous mass as a unit, with power to assume the form of encephalon and cerebro-spinal axis, sympathetic system, ganglion, plexus, nerve-fibril, or nervous-molecule: wherever it is, it is the unit nervous mass, all whose items are bound into one co-ordinated whole by the centralized power existing in the medulla oblongata.

This unit-mass may extend or protract itself, and evolve itself into the form of optic-nerve and retina, lung, gland, spleen, hand, heart, ear, alimentary apparatus, skin, muscle, capillary vessel, joints; in short, it extends itself and becomes organ, in order to maintain itself by perpetual supplies of alimentary material and oxygen. The organs and all the parts are its machinery, to which it supplies the necessary power, and which it withholds from them when it is itself become incapacitated.

If the nervous mass is thus the regent and supreme dominator of all the parts, it can only be so when the necessary conditions of its own normal existence are present; but the nervous mass is subject to induration, to ramollescence and pressure—to too little pressure—to strength—to weakness—to various pathological states, and to death. To point the finger of scorn at a man, arouses in an instant the fiercest passions of his soul. To blast him by some spectacle of horror, is to make every fibre tremble and quiver. To appeal to his human sympathy, is to melt him into the softness of the woman or child. To heat him with wine, to anæsthetize him with chloroform, to cast him under the therapeutical force of antimony, or mercury, or opium, is to

alter the state and force of his nervous mass. To place him, in autumn, near a malarious swamp, is to bring his nervous mass under the altering influences of the malaria: but, to deliver a woman in a lying-in ward filled with the cause of childbed fever, or in a town or district wherein the cause of childbed fever is become, by some constitution of the air, epidemical, is so to modify, by means of the malaria of the town or ward, her nervous element, that, after delivery, she shall give manifestations of this morbific power in a peritonitis, or metritis, or metrophlebitis.

Let not the Student invite me to show what are the modifications of the nervous mass, brought about by the ward-malaria. I know them not—any more than I know the modifications of the encephalon effected by the operation of the causes of rage, terror, sympathy, or heaven-descended charitableness that divides its cloak with the beggar. *Medicina non agit in cadaver* is a true saying. What modification is it of the nervous mass, that leads to the therapeutical effects of ipecacuanha or tartar emetic doses? Again I say, *medicina non agit in cadaver.*

As to peculiar states of the nervous mass brought about by means of the epidemic cause of childbed fever, their existence will not be denied by the contagionists—who, at most, only accuse physicians and nurses of transferring, by their person or dress, the *cause* from patient to patient. These very contagionists contend for some identity in the causation that renders the congener, erysipelas, rife during childbed fever epidemics. But they contend not for the contagion of this erysipelas.

Contagionists will hardly contend, that an incubation of a few minutes can suffice to awaken the fatal train of symptoms—but, and here is the rub, in epidemic childbed fever, women are sometimes attacked, sickened, and destroyed within twenty-four hours, or even within eight hours, after the close of the labor. Kiwisch has observed this frightful rapidity of dissolution. Puzos and many others have seen cases to prove fatal in twenty-four hours. Epidemic constitutions of the air may so affect the state of the nervous mass, as to give a wide-spread reign of synocha, or typhus, or yellow fever, or plague, or cholera, or black death, English sweat, or childbed fever, whether synochal or typhal: during the reign of a great epidemic, only those that fall sick appear to have been brought within the range of the causation. Yet it cannot be doubted that, of a population consisting of 100,000 souls, among whom 5000 only manifestly fall sick under the epidemic, the multitudes of the population who have resisted the cause, though exposed to it, greatly exceed in number those who have been disordered by it. In a crowded lying-in hospital there are many

servants, and pupils, and physicians, and other officers. There are also many unmarried women, as, for example, Sisters of Charity, who wait on the sick. Ofttimes the lying-in wards are nigh to fever wards, or wards for the wounded. How is it, then, that this potent contagion, which destroys like the most virulent poison, even in a duration of only eight hours—how is it, I ask, that other human beings, females and males, are never even suspected to be in danger, while a pregnant or lying-in woman is looked upon as in the greatest peril while she stays in that house, breathing the same air, living on the same food, and in all respects, save her pregnancy and lying-in, in like circumstances with the other non-pregnant women, men and virgins by whom she is surrounded, and who maintain their perfect health. If the contagion is a contagion, it must be a specific one, and very curiously specific, if it specifies only the *accouchée*. How can the Student regard it as specific in this sense? How can he, in fine, hold it to be a contagion? Is there a contagion specific to the pregnant woman, and of no power over the bodies of other women, of children and males? As well might the Student say that pregnancy has changed and subverted the specific character of the woman. It would be nonsense to say so. Yet if he be a contagionist, he will persist to say that a doctor who is a gentleman, and who cannot by any means be made sick by this contagion-cause, cannot come near a pregnant woman to feel her pulse and make her poke out her tongue, without killing her!

But, if the Student should incline to say that the cause, though it may have been originally generated within the house, as marsh-miasma is generated in the swamp, is yet capable of reproducing itself in the economy of the sick woman, and so become contagious; nay, that it may adhere to and even be transported by her medical attendant to considerable distances, he ought, at least, to say that time is required for the operation of the cause. Such operation cannot be instant, and without incubation. Smallpox, whether taken by ordinary contagion or by inoculation, and syphilis also, must have an incubative stage. But, in our disease, the woman may be attacked and lost before the lapse of twenty-four, or even eight hours. Will he still incline to say, that the physician carried the poison from the last patient and killed the woman in eight short hours? It seems to me that this statement serves to show that the woman must have been brought under the range of the cause before the commencement of her labor; and, if so, then certainly not by any infection conveyed by the attendant, but only by the epidemic force which may have been acting upon her nervous mass for many days. How many days? Shall we say one

day, or a week, or a fortnight? But, the cause of epidemic childbed fever covers, like an overshadowing cloud, a whole city, state, or continent. It may have silently operated for a month or more before the explosion.

Is it not idle, then, to look to human contagion as the cause, when we know of a surety that epidemic constitutions of the air may comprehend within their morbific scope thousands or millions of the population who shall not in a single instance sicken under it, except after the application of a proper and competent exciting cause?

I repeat that as the epidemic-cause of childbed fever cannot affect any others than women, pregnant or lying-in, it must, therefore, be a feeble cause—else it would produce disease in unmarried women and girls, as well as males. Still, it is powerful enough so to affect the nervous system of pregnant and lying-in women as to give them a propensity to be seized with these puerperal phlegmasias. The three forms of childbed fever are each pure forms of non-specific phlegmasia. They are terrible in their rapidity and mortality, because the lying-in state requires only that a disk, an area of inflammation, should be once established; which being done, the area expands like the circles of wave in a lake wherein one has cast a pebble: the motion is propagated to the shore.

It is not improbable that states of the mind brought about in pregnant women, by rumors of numerous fatalities, may exert an injurious influence on the nervous mass, which becomes weakened and disordered by states of the soul, inviting to the attack. I heard of a soldier who was sick in Mexico. He longed to return to the United States. The physician who examined him found him little indisposed. The soldier had the keenest desire to visit his home, and said, "Will you let me go home, sir?" Being roughly refused, he turned his face away, and was dead in a few moments. Now here was a case in which a word conveying a bitter disappointment caused the nervous mass of a man to lose its power over the organs. It resigned its authority over the dominions it had controlled, and a momentary anarchy led to their total dissolution. Is it wonderful, then, that a woman, whose ears are filled with terrifying reports of epidemic childbed fever, or whose eyes have witnessed the dissolution of numerous inmates of her own hospital-ward, should allow a non-specific inflammation to fix itself upon the womb, or ovary, or venous sinus, whose area, augmenting like the wave-circle in a calm lake, should speedily reach and disturb every part of the economy?

For the most part, a woman who has been confined, and who has got quite rid of all the uterine products of the gestation, remains in a

quiet and comfortable condition as soon as she shall have somewhat recovered from the fatigue of the conflict, and got over the smarting and aching sensations left by the transit of the child through the organs. If she be confined for the first time, she is little apt to suffer from those after-pains, with which others are troubled who have borne children before.

The blood shed by the empty womb, flows out upon the napkins placed to receive it, and a few small coagula formed within the uterus or vagina are in good time expelled; and the pulse, the respiration, the temperature, and psychical state of the woman present no motives why we should apprehend any attack of disease. Twenty-four hours pass in this quiet way. Perhaps the lower belly is somewhat sensitive to the touch, but it ought to be a little so. She takes light food, and convenient drink. She is kept neither too cold, nor too warm. She does not leave the bed, nor even sit up in it. She sleeps well. She applied the infant to the breast, and has given a little milk. Forty-eight hours pass in the same favorable way, the milk gradually increasing. She sleeps during the first part of the second night, but at four o'clock in the morning she awakes to nurse the child, and then she observes that there is a degree of soreness or painfulness in the hypogaster, as she turns to adjust the breast. She becomes chilly: a rigor of positive character comes on, which sometimes amounts to an ague-fit, with chattering of the teeth, trembling, and a sense of distressing coldness along the spine: the fingers, toes, ears, and nose are cold to the touch, and now the pains in the uterine region become severe. She no longer can bear to have her binder pinned tight, or suffer the nurse's or her own hand to be pressed upon the part. Sometimes these pains are so violent as to draw cries and even screams alternated with moanings from her lips. She lies upon the back, and draws up the knees, for she cannot extend the legs without increasing the pain in the lower bowels. If she have occasion to change her posture or turn in bed, she does so with great precaution, because the least change of posture augments the distress. If she happen to turn upon the side, she seems to feel that something within falls heavily towards that side, and draws or drags other parts painfully along with the heavy, and fleshy, and sensitive womb. If she should happen to cough, the agony occasioned by the effort and shock is unspeakable.

In half an hour, in an hour, or in two hours, less or more, the chill is gone off, and the face is become red; the hands and feet are hot; the respiration is quick and short. She can not dare to take a full breath, because, in doing so, the great descent of the diaphragm too rudely and painfully moves the parts beneath it downwards, towards,

or against the sensitive areas of the phlegmasia, which can tolerate no great disturbance nor rude touching. Hence, she breathes with a short, quick, frequent respiration.

If, during the rigor, the pulse is counted, it will be found beating one hundred and twenty, or perhaps one hundred and forty times to the minute; the artery being small and wiry, with a quick and sudden stroke of the systole. As the chill goes off, the pulse increases in size, but lessens a little in frequency, as the hot stage begins to develop itself. The hot stage makes progress; the area or inflamed disk expands, and, as it does so, causes the woman to experience frequent slight, momentary rigors, that are insufficient, however, to arrest the progress of the hot stage. In proportion as the area of phlogosis enlarges, so does the constitutional reaction against it augment. The effort of the heart becomes more and more energetic; the systole is again more frequent and sudden; the temperature of the body increases in the same ratio, and a hot stage is soon fully established. During the excitement of the hot stage, the area of inflammation expands rapidly.

Under the predisposing conditions arising out of the woman's puerperal state, this reaction spreads out the area of inflammation more and more until it transcends the boundaries of the pelvis, where it was first established; it migrates upwards on the peritoneum covering the iliac muscles; it inflames the serous coat lying behind the muscles of the belly; it creeps up along the meso-rectum, and the sigma of the colon; it burns like a raging fire in the loose and movable peritoneum of the omentum: the whole colic arch is inflamed; the meso-colon; the mesenteric and intestinal peritoneum are involved; it takes hold on the stomach, passes along the serous lining of the liver, and fastens itself on the vault of the diaphragm, and the abdomen becomes tympanitic almost to bursting. Under the overthrowing power of such a complication of inflammations, the nervous power is demolished. The belly has become tympanitic; the respiration and calorification are disturbed and lessened; and the blood becomes more and more venous in its character, as the neurosity grows less and less under the lessening aëration of the blood. The countenance and the skin undergo the remarkable changes so often signalized. The stomach gives way with eructations, regurgitation, vomiting of yellow, green, blackish fluids, and lastly black-vomit. The nails are bluish, and the lips also; all pain has ceased; the pulse grows small, thready, vermicular; it ceases at the wrists, elbows, and axillæ, and at last the heart lies still; the woman is dead, the child an orphan, the family altar is overthrown as the divinity that presided there takes its flight to the unseen world.

This is a picture of the progress of a childbed fever assuming the form of puerperal peritonitis. The sketch I have presented is a rapid one; but it seems to me that many women have begun and matured the whole progress of the events I have related, in a little more time than I have employed to write these words. In common, however, the rate is not so rapid. As a general rule, my cases have begun a little later, as at the end of the third day; so that I suppose the seventieth hour is more likely to give rise to the first symptoms than the forty-eighth hour after delivery. Still, it is not to be denied that, in multitudes of women, the area of phlogosis is first laid during the labor, or even before the commencement of it.

I hope the Student will allow me now to remind him of what I have before said, namely, that the serous coat of an abdominal viscus is an essential part of such organ; and that inflammation of its serous coat is, virtually, inflammation of the organ itself. But in my statement of the progress of the disorder I have asserted that the original area of the peritonitis has become expanded, or that the inflammation has migrated beyond the bounds of the pelvis, and has at last fastened itself upon each and every of the abdominal viscera and the diaphragm. This I regard as a true account of any one of the bad cases of peritonitis. The Student would expect, even were the area of inflammation confined to the limits of one broad ligament, a very hazardous constitutional reaction from it; what shall he expect when every viscus is become inflamed in the progressive expansion of the area!

When the whole peritoneum of the colon and small intestines becomes involved, the peristaltic muscles, lying underneath their serous coat, must lose their power, and the whole tractus of intestine become distended with gas, just as a paralyzed bladder suffers itself to be filled to bursting with urine upon which it cannot contract to expel it. In like manner, the inflamed bowel fills with gases extricated within them, so as to produce, first, a state of meteorism which soon becomes an enormous tympany. The woman's abdomen begins to expand, in a short time after the inflammation has ascended upon the bowels, and then speedily becomes larger than it was before the birth of the child. I have seen some women in whom the abdomen was tense as a drum-head, and perfectly sonorous under percussion.

The nervous system ought very soon to succumb under the torment of such a vast area of inflammation; but there is another influence that tends quite as rapidly to sink it under such circumstances; it is this: the diaphragm has not free play any longer, to descend, as the respiratory piston, to the proper point in the cylinder of the trunk; for whenever the woman would make a free aspiration of air, she is

prevented by the pain; for as the diaphragm goes down, carrying everything beneath it in a downward direction, its pressure upon the inflamed bowels gives rise to torturing pain, so that she will not, if she can, and cannot if she will, take a full and perfect breath. To breathe imperfectly, is to oxygenate the blood imperfectly; and hence, this great and painful tympany soon comes to interfere with the aëration of the blood, and consequently with the innervative power which depends upon it. The constitution thus sinks rapidly under the double influence of a commencing tendency to asphyxiation and an intolerable burden of perceived irritations. All the writers notice a peculiar expression of the face in women in childbed fever. The countenance has a peculiar leaden hue, which it acquires in consequence of the imperfect aëration of the blood just explained; and this it is that gives the peculiar childbed fever physiognomy, so much spoken of in the books.

Moreover, when the bowels become thus greatly distended, their superficies of inflamed serous membranes expands *pari passû*. There is little hope, therefore, to effect the cure of the serous inflammation by resolution; it can come to its term only by effusion and adhesion, or by gangrene. The latter result is not to be expected, since the woman must die with an irritation so terrible, even before it could reach the point of passing into gangrene.

In some women, as soon as the bowels have become thus greatly inflated, a total stoppage occurs by what I have called angulation of the gut. For example, if the colon should be greatly distended, its returns, instead of being effected by arcs, are effected by angles; but a hollow cylinder, suddenly bent at an acute, or even right angle, will shut its cavity at the angle, so as not to allow even gases to pass. If the Student would roll up a sheet of paper into the form of a hollow cylinder two inches in diameter, and bend it so as to make an acute angle at the return, he will find that he cannot even force his breath through it. This is what happens in some of these cases. The obstruction becomes absolute and complete, so that neither liquids nor gases can pass; and the case becomes an iliac passion to all intents and purposes, and the woman dies as surely as if a ligature were applied and strictly tied around the gut. I have met with such instances, and proved them by dissection. No medicine can operate. The largest doses of calomel, senna, jalap, salts, croton oil, or elatin, are alike unavailing, for such angulation of the gut is a mortal accident. I saw a woman, attacked at 4 o'clock A. M., who at 11 A. M. was dying, under these very circumstances. It was in vain that any efforts to rescue her were made.

While these consequences flow out of the serous inflammation of the intestines, what must be those resulting from the serous gastritis? Every person who has witnessed the cases must be familiar with those fatal signals that are early presented in the first gastric eructations of thin mucus mixed with ingesta; soon after which appears a yellow-tinted fluid, that comes at last to be greenish, verditer, dark green, darker, black and granular, and at last black vomit. These are the signs and fruits of an inflamed and dying stomach and duodenum.

Why need I further cite the serous hepatitis and paraphrenitis? It is enough to say that we have allowed the area of phlogosis to expand, to mount above the plane of the superior strait, and invade, as a fire in a prairie, every tissue that can be a fitting prey to the flame!

The Student has here, as I hope, acquired a clearer view of the nature and progress of a case of childbed fever, under the most ordinary form of puerperal peritonitis; and what I most desire is, that he should solemnly reflect upon the idea of an area of inflammation beginning within the pelvis, of a small superficies at first, but allowed by him to transcend its original limits and at last pervade everything within its reach. I feel sure that he will not look with surprise upon the woman who, under such circumstances, seems to sink without a struggle for life, her functions perishing, one by one, until the medulla oblongata at last ceases to excite the motor cord of the vagus nerve, which is death.

In the progress of the various operations I have depicted, the woman reaches a stage of the inflammation in which nature attempts the cure by effusion. As soon as the serum and sero-pus, and copious albumen begin to exude from the serous surfaces, the pain is mitigated, becomes rapidly less and less, and is soon all gone. The belly still continues to be large. If angulation has taken place, it will never subside; but if not, then, when the effusion has occurred, the abdomen becomes softer and less resonant, but still continues much distended. The inflammation is at an end, and it has come to its term by means of the act of effusion. By percussing the abdomen, we can now discover that the peritoneal sac is filled with a fluctuating liquid, and we know that the signs of melioration, so joyfully hailed, are but the disguised harbingers of doom. If the Student should read Gordon's account of the case of John Low's wife, he will have a most touching picture of the treacherous nature of this enthanasial melioration. When the effusions began, her great pain ceased, and, upon the doctor's arrival, they "received him with transports of joy;" but he saw that his patient was dying.

The Student ought very carefully to obtain a correct diagnosis of

the condition of any woman recently delivered, who complains of indisposition. If he should make an early and correct diagnosis of the incipient areas of inflammation, which I have supposed to be the true beginnings of childbed fever, he could scarcely fail of success by a resort to the Gordon method, to be hereafter described. And I advise him to give his patients the benefit of any doubt that may be left after a due exploration of the signs; that is to say, if he remain in doubt whether the malady be inflammation or not, let him proceed with his method as if justly convinced of the existence of a veritable inflammation.

One of the most constant signs is a remarkable frequency and quickness of the pulse, with which, however, he cannot become familiar by mere book learning. One must feel the childbed fever pulse in a few cases in order to become familiar with the peculiar and informing impression it makes upon the sense of Touch and the mind. I shall no further attempt to describe it than by saying that it generally beats at least 120, and often 140, or even 160 strokes to the minute. The volume and resistance of the artery vary, even in the early stages, according to the constitution of the patient, the violence of the phlegmasia, its extent, and the force of the epidemic cause, or other cause.

Let the diagnosis between milk-fever and childbed fever be made with care: milk-fever may be accompanied with after-pains, with rheumatism of the womb, or with pure neuralgia of the pelvic and abdominal viscera, strangely connected with a tenesmic condition; or with retention of urine, or with retained coagula, or costiveness. I have met with many cases of supposed childbed fever, for example, that were by no means childbed fever, but only pain and irritation in the pelvis and belly, and a quick pulse and hot skin, caused by a tenesmic descent of the late gravid womb. In such cases, the abdomen is as tender and sore as it is in the most violent peritonitis, but it is instantly relieved by pushing the uterus upwards with two fingers in the vagina. If there be doubts about it, let them be cleared up by raising the womb upwards in the pelvis, and then by striking upon the sore abdomen to learn that the operation has wholly taken away all the pain and soreness, which it could not do if they were the symptoms of a real phlegmasia, instead of a descent of the womb. But, as I cannot devote, in this volume, a space sufficient for a treatise *in extenso* upon our disorder, I shall rest content with having indicated the above-named subjects of diagnostic research.

What can we do in these cases of childbed fever? What is our duty? Who shall show us any good? The profession is utterly di-

vided upon the treatment that ought to be adopted. And I am quite sure that what I shall recommend will meet with reprobation in many quarters. Yet there ought to be, and there must be, a reason and a philosophy that ought to guide all scholars upon a question that seems to be so simple. Instead of this unity of thought and purpose, we are divided; and the Scriptures tell us "that a house divided against itself cannot stand."

I may venture to enumerate the following as the principal remedies in vogue for the cure of such cases as I have described:—

1. Bloodletting; 2. Leeching; 3. Cupping; 4. Emetics; 5. Cathartics; 6. Stuping and poultices; 7. Blisters; 8. Turpentine-oil; 9. Opium; 10. Mercury; 11. Tartar emetic; 12. Enemata. These are all that are in common use. In order to seek for some choice of method, I shall inquire *seriatim* into the utility and reliableness of each of them.

Bloodletting.—It appears to me that whosoever shall accept those views of the nature of childbed fever that have been herein set forth, must also regard the use of bloodletting as an indispensable element of any method of cure for the disorder. The Student, who truly desires to become acquainted with this great subject, will have examined the history of medicine, and will have made himself familiar with the literature of the subject. He will have learned that, so far back as the age of Hippocrates, the abstraction of blood was relied upon as the most hopeful and safest recourse; and that, throughout the lapse of the centuries of medical history, there have always been some physicians to trust in chief to this remedy. It could not well be otherwise with men who had taken advantage of occasions to examine the nature and extent of the inflammatory lesions exhibited in the dead body. Eucharius Rhodion, Mauriceau and Guillemeau, Lamotte, Puzos, and many others, were persons who had clear views of the phlogosis of childbed fever, and they became in consequence good bleeders; bleeders *coup sur coup*, as Puzos expresses it.

The notion, the false notion, that the disease is a fever, still continued to mislead, as it still does, many good and wise men; and even of those who clearly perceived the phlegmasia, not a few appeared to have been dominated by the notion of a fever to that degree, that, while they boldly advocated venesection in their writings, they became appalled in presence of the cases, so that their practice, as in Leake's case, and Denman's, fell far short of their promises and expectations.

It chanced that, in 1789, an epidemic childbed fever began to prevail at Aberdeen, and in the neighboring villages and districts, which proved fatal to many women. None of the medical men then in

practice at Aberdeen were acquainted with the disorder, though the same epidemic had been fatally prevalent there in 1760; but the practitioners of that date were now all gone off the stage.

At that time there lived in Aberdeen a physician, Alexander Gordon, who was largely engaged in Midwifery practice; he was a person of sound judgment, of great probity, and truly zealous for the truth. The cases that came under his care proved fatal, one after another, so that out of the first seventeen women whom he attended, he lost nine. Gordon had not cleared his understanding from the pernicious influences of the traitorous word fever; and therefore, through all the tribulation and distress of so many losses, he was never enabled to behold the simple truth of the case, until, upon the death of Isabel Allan, his seventeenth patient, he procured permission to examine the dead remains. The following is his description of what he observed. Let the ingenuous Student read and carefully reflect upon it, with the view to learn what could, what must have been the state of the serous investment of the belly, that could have led to such effects.

"Upon opening the abdomen," says Gordon, "I found the peritoneum and its productions, the omentum, mesentery, and mesocolon, in a state of inflammation. The omentum had lost about half its substance by suppuration. The mesentery and omentum, and all that part of the intestinal canal with which they are connected, were very much inflamed. But the disease appeared more especially to occupy the right side; the right ovarium had come to a suppuration; the colon, from its caput along the course of the ascending arch, was much inflamed and beginning to run into gangrene. A large quantity of pus and extravasated serum appeared in the cavity of the abdomen, which, when taken out and measured, amounted to two English pints. The peritoneal coat of the uterus was inflamed, and the organ itself not so compact and contracted as it ought to have been. Upon opening it, its cavity was found covered with a black-colored substance, which at first had the appearance of mortification, but, when wiped off, was nothing else than the membrana decidua in the state in which it naturally is about this time."

The truth here flashed upon Gordon's mind. A thousand speculations, ten thousand essays like Hulme's, centuries of fixed opinions that childbed fever is a pyrexia, weigh less than nothing in counterpoise of these proofs; that it was a phlegmasia Gordon now knew.

Henceforth, throughout the remainder of the Aberdeen epidemic, which as I have said began in 1789, and prevailed until 1791, Gordon walked in a path of light. He emerged from the gloomy and doubtful track along which he had previously stumbled and groped,

amidst clouds of error, and accompanied with phantoms of the imagination to mislead and baffle him. He saw that the disease is an inflammation, to be combated by antiphlogistics, of which the chief is bloodletting. But he rightfully judged that, to bleed merely, is not to cure; for he judiciously resolved, in using the remedy, so to apply it as to kill the malady at a blow—*jugulare febrim*. He considered that, like the fabled hydra, its multitudinous heads must be stricken off at a blow, and that, if struck off one at a time, they were only the more speedily reproduced.

Gordon says: "The method I proved most successful was, by copious bleeding soon after the attack of the disease." * * * "When I took away only ten or twelve ounces of blood from my patient she *always died;* but when I had *courage* to take away twenty or twenty-four ounces at one bleeding, in the beginning of the disease, the patient never failed to recover." * * * "If, therefore, a practitioner is called to a patient in the beginning of puerperal fever, he must never take away less than twenty or twenty-four ounces of blood at one bleeding; otherwise he will fail in curing the disease." "I know that this will be thought too large a quantity, by those who never take away more than eight or ten ounces of blood from their patients; but such practitioners would never cure puerperal fever; for, unless a practitioner venture to take away the quantity mentioned, it would be much more prudent in him not to bleed at all, because his patient will certainly die, and the bleeding will be blamed." * * * * "And I felt this prejudice in its full force, when I had not courage to take away more than ten or twelve, or fourteen or even sixteen ounces, of blood from my patients. But when I had resolution to take twenty or twenty-four ounces at one bleeding, I disregarded it, because I was sure that that quantity, taken away within six or eight hours after the attack, would certainly cure the disease, and that of course there would be no clamor against the bleeding. But, when I was not called at the beginning or soon after the attack, I did not bleed at all." * * * * "Now, when I was called early to patients in the puerperal fever, and had courage to take away twenty-four ounces at one bleeding, I never failed at once to cure the disease." * * * * "After much experience in the disease, and mature deliberation concerning the conduct most proper to be pursued in my peculiar situation, I came to the following resolution: If called to a case within twelve hours after the attack, I insisted on bleeding the patient, and promised for its success; but if at a later period, or from twelve to twenty-four hours after the attack, in that case, like Sydenham with the same remedy in the smallpox, I

thought it incumbent on me to propose it as the only effectual remedy, but I neither insisted on it nor promised for its success."

Many valuable publications upon childbed fever have appeared since the date of Gordon's work. It is not very long since I procured to be published, through the approbation of Dr. John Bell, now Professor of the Practice of Physic at Cincinnati, a volume containing, first, a short article on puerperal fever, by myself, designed as an introduction to the rest of the volume, which consists of the works of Gordon, Hey, of Leeds, Armstrong, of Sunderland, and Robert Lee, of London, on puerperal fever. I have considered that the doctrine of those four authors, as to childbed fever, is so clear, and so clearly descriptive of the facts, and the treatment by bloodletting, set forth in a light so true, that it might serve as a sufficient text and guide-book for the brethren in this country, many of whom cannot have access to rich libraries of medicine. I am still of the opinion, that all those who shall truly possess themselves with the contents of that collection will be well informed and safe men. Safe, I mean, for the public.

While I so confidently recommend the above-named authors, I still more sincerely wish that all the American accoucheurs could read the essays of Tonnellé, of Legouais, of the younger Baudelocque, of Meissner, Kiwisch, and other good writers, who have seen the truth and told the truth as to the phlogistic nature of the malady, and the necessity there is for an antiphlogistic method. I feel sure that few reasonable, well-thinking men could read them without coming over to our opinion. As to the statements contained in the above extracts taken from Gordon, I subscribe to them as the truth, pure and simple, for I know them to be true as to my own experience. His experience amounted to seventy-seven cases; mine goes far beyond that number, and all that I have saved have been saved by the lancet. I am indebted in chief to Gordon for the privilege of standing on a platform, from which I can look over and beyond the mere signs, and discover the things, which they but outwardly represent. In the failing pulses, and amidst all the disheartening evidences of the typhal state, I can look upon the expanding area of the inflammation as the cause, and the sole cause of the declension of the forces, and the overthrow of the functions. If I cannot cure her by venesection, my patient may recover by the providence of God. All other human means seem to me to be useless and beneath contempt, as prime remedies, venesection being omitted.

Hence, my advice to the Student is to learn to confide in venesection as the sole therapeutical hope; but, in the meanwhile, continuing the

other remedies, which are but the juvantia and adjutoria of the masterful and hopeful chief and leader in the conflict against this dreadful, most fatal disorder.

Gordon points out twenty or twenty-four ounces as the proper quantity of blood to be drawn in the early stages of the malady. Probably, he has in this hit upon the happy medium, and I most warmly advise the Student to follow him. Still, the question as to a second venesection will often arise, and I am by no means willing to say that another bleeding shall be regarded as imprudent at a later stage. It is clearly incompetent for any man to determine, beforehand, that twenty-four ounces shall be sufficient in all constitutions alike to effect the cure; and certainly some women may bear, and even require to have taken away thirty ounces, or even a larger quantity. What I most desire in the Student, and what I thank Gordon most for, is the earnestness with which he fixes the lowest limit at twenty ounces.

Let the Student read Hey, Armstrong, and Lee, Legouais, Baudelocque, Dance, Meissner, and Kiwisch, to discover more clearly, if possible, the grounds for defending the antiphlogistic treatment.

It is true that this treatment is combated in very high quarters, and among others by that most excellent and worthy writer, Dr. Robert Collins, of Dublin. This gentleman's experience in the treatment of hospital cases, which was considerable, led him to the opinion that bleeding is not the proper remedy for those cases, however much it may be applicable in out-of-door patients. He says, "at least, in *four* epidemics which I have witnessed, the symptoms were usually of the lowest typhoid description, the pulse being so feeble and indistinct, as to make you dread, in many, even the application of leeches," &c.

Dr. Collins had charge of eighty-eight cases, of which thirty-two recovered, and fifty-six died. Only fifteen of the eighty-eight were bled, of whom seven recovered, and eight died. He adds: "The result of my observations upon the treatment of puerperal fever is, that general bleeding, except when there is a strong full pulse, and the symptoms are of a highly inflammatory character, is injurious." Such are the words of Dr. Collins, from which it is plain that he entertains views of the malady different from those I have in this chapter set forth. I look upon the cases of peritonitic form as being invariably dependent upon a primordial area or disk of inflammation, which ought to be limited and then *resolved* by early venesection.

Upon examining Dr. Collins's cases of fatal disease, the No. 60, page 424, gives us the example of a woman delivered at 4 P. M. on the 12th. On the 13th, had slight pain in uterine region; pulse quick; an oil draught. In the afternoon, pain greater; soreness; hurried pulse;

moist white tongue. (The case probably installed early on 13th.) Four dozen leeches, &c. On the 14th, 9 A. M. (say twenty-four hours after the attack), she was in great pain, on account of which four dozen leeches had been applied at 1 A. M. Twenty-five ounces of blood were taken from the arm, which "she bore extremely well," though the venesection did not in the least relieve the pain.

At 9 P. M., the pulse being *full* and beating 138, she was again bled twenty-two ounces, which, with the twenty-five ounces before drawn, makes forty-seven ounces of blood; besides what was taken by four dozen leeches previous to the first bleeding, and two dozen soon after the second bleeding. On the 15th, the pulse 108; on the 16th, 108; 17th, 120; 18th, 120; 19th, 132; 20th, 138; on the 21st she died, at 2½ P. M. In the course of the treatment, from the 12th, the day of her delivery, to the 21st, at 2½ P. M., say nine days, she lost forty-seven ounces from the arm, and had fourteen dozen leeches, four hundred and sixty grains calomel, and twenty-three grains of opium, five warm baths, constant stupes, and occasional draughts of castor oil and turpentine.

The above is an abridged statement of case No. 60, given by Dr. Collins; and I now beg leave to ask the Student to consider whether, inasmuch as the patient bore such large bleedings without sinking, and as she held on in the conflict during nine days, it is, or is not probable that Gordon's method, fully and fairly carried out, would have saved her life. She was delivered on the 12th, at 4 P. M., after three hours' labor. On the 13th, A. M., slight pain in the uterine region; pulse quiet; but at 4 P. M. pain increased, *much* sensibility to pressure; pulse hurried. It was not until past 9 A. M. of the 14th, more than twenty-four hours subsequent to the establishment of the intra-pelvic area of inflammation, that the twenty-five ounces were drawn from the arm. Gordon advises us to bleed within six hours, and says we always cure, and I believe him. Nor can I doubt that No. 60 would have been made quite safe, had the venesection been performed at 9 A. M., or even at 4 P. M. of the 13th. Let the Student observe, further, that this was an *hospital case.*

I entertain so great, and so sincere a feeling of respect and veneration for the character of Dr. Collins, that I am prompted to omit the foregoing observations upon his case, No. 60, lest he might misapprehend the feelings that have led me to remark upon it. Dr. Collins is a person far more deservedly eminent in our art than I can ever hope to become, and his services to our science have obtained for him the applause of the whole Republic of Medical Letters. I trust, under these circumstances, that he will pardon me for selecting a case from

a so high authority, by which (if haply I might succeed) to compare my own views of our duty and policy, in these trying difficulties, with his own. And I must aver that the case in question, coming as it does even from so eminent a source, cannot but confirm me in my adhesion to Gordon, Lee, Armstrong, Hey, Legouais, as well as to the multitudinous convictions of my own clinical opportunities, both in hospital and in town. I ought, however, to be very careful not to conceal any part of the grounds on which Dr. Collins rests. Those grounds are plainly set forth in his work, it is true; but, as some of my readers may not have it in their power to consult the *Practical Treatise on Midwifery*, &c. &c., by Dr. Collins, London, 1836, 8vo., I shall here insert extracts from a letter from that gentlemen, which he did me the honor to write in the month of January, 1849.

"MERION SQUARE, DUBLIN.
"*January* 2, 1849.

"MY DEAR SIR:—

* * * * * * *

"At pages 609–10 (*Woman and her Diseases*), you compare the mortality in puerperal fever under my treatment, and that of my distinguished friend, Dr. Robert Lee, of London, to prove the greater success, where general bleeding had been more frequently adopted by him.

"The GREAT and MARKEDLY DISTINGUISHING FEATURE between Dr. Lee's cases and mine, has, however, been overlooked; as mine were all HOSPITAL PATIENTS; whereas, his were all treated at *their own dwellings*. This was also the case with the late Dr. Gordon's patients, to whom you so deservedly allude.

"The disease with us, and I believe universally, is as different IN hospital and OUT of hospital, as it is *possible* to *imagine*.

"Please look to my observations, pages 390–1–2, &c., where I have stated the patient to be little more than SHADOW, and to exhibit the appearance of those laboring under *cholera;* so as to make us *dread* even the application of leeches. The fever is of the LOWEST TYPHOID character, with the pulse *so feeble* and *indistinct*, as to totally prohibit general depletion. This form of the disease is singularly intractable and truly fatal; whereas, the inflammatory form of puerperal fever, such as usually met with OUT of hospital, may be treated with considerable success.

"I should have stated that few physicians have witnessed the results of general bleeding to *a greater extent* than I have done; as the master of the hospital who preceded me, and to whom I was *Assistant*, was a STRONG ADVOCATE for it; but the mortality was so frightful, he was

forced to abandon it. He bled *instantly* and COPIOUSLY, but with the most fatal results.

"Such is the character of almost all our *hospital* epidemics."

I am sure no man will have the hardihood to pretend that bloodletting is a cure for childbed fever. Bloodletting is not a cure, but a method, and only a method. There will, of necessity, be met with many cases which the method cannot cure. Still, it remains *the* method, and is preferable to all others, and, moreover, so very likely to cure, when duly and timely applied, that I cannot conceive of any other to be compared with it for excellency and safety. Indeed, I reiterate that the case which admits not of this method is one to be cured through the godness of Divine Providence, and not by the vain means left in our power after the subtraction of the prime and chief of them all. Bloodletting is the most manageable of methods. Nothing equals its power to control the circulation and the innervation, whether those of the whole economy or those of the diseased areas.

If the area have already comprehended the whole fundus uteri and broad ligament, a state expressed by rigors, by sensibility to pressure, pain, and febrile reaction, with the pulse at 120 or 140, surely there is nothing that man can do to equal in efficacy those states of the circulation, calorification, impressionability, and motivity, that he can bring about at will, with his eyes on the patient, and his fingers on the pulse, while he lets the venesection proceed. He can stop the jet now, or in half a minute, less or more. He can read the impression he has made upon the nervous, the circulatory, and motor forces, in their indexes in the countenance, in the pulse, the heart, the pain, the respiration; and I have no doubt, if a man will take a true, and a dispassionate view of the present and the future of the case, he shall, in the majority of instances, yea, as often as Gordon asserts, wrest the victim out of the hands of the destroyer. But he shall adopt no half measures. He shall not preach copious bleedings with Leake and Denman, and practise small ones like those gentlemen. He shall shut his eyes to the weakness, to the faintness, to the sinking, to the typhus, and open them to comprehend the real extent of an area of inflammation, which, if not incurably expanded, he may hope, or try to cure, but which, with Gordon, he abandons to the powers and resources of nature and Providence, when he deems it too great for his method.

Such are my views, not taken up at hap-hazard and carelessly, but pondered upon and acted upon for more than the third part of a century, in the midst of incessant clinical experiences. I shall never cease

to advocate them, as I conceive them to be founded upon true principles in physiology and pathology.

Therefore, I say to the Student, if he have a patient in charge attacked with childbed fever in the peritoneal form, and if he can come to the bedside soon after the onset, the sooner the better, let him bleed if possible twenty-four ounces, or more, and not less. Let him say, if compelled to desist with the abstraction of twelve, or even fifteen ounces, "She is not safe!" but with twenty-four ounces or more, with an incipient and not too large area of phlogosis, "*She shall* be saved by the method."

In case No. 60, from Dr. Collins, on which I lately presented some remarks, that gentleman, as has been seen, repeated his venesection, and, in so doing, gave his patient the best chance that could have then been presented to her. I admire the boldness of his proceedings in that instance, and I should be happy, could I think the Student would hereafter imitate him in that resolute prosecution of the method. I, of all things, in this relation, most earnestly wish that he would apply the method timely or early; but if he should not arrive in time to do the operation early, it may yet not be too late to do it. There is a stage at which the method ought not, and will not be applied by any sensible man. When the inflammation, for example, is upon the point of ending in effusion, or when the effusion is already in progress, to bleed can do nothing but mischief. As a general rule, he will be obliged to follow Gordon, who, if twenty-four hours had elapsed, felt obliged to propose bloodletting, though he could make no promises of good from it. This rule of twenty-four hours is sound for the general, but is not so for every particular, for there is no ascertained ratio of progress; whence it might and will happen that a venesection may not be too late on the third day, or even on the fifth or seventh days. Such instances will, however, be rarely met with.

To show that weakness existing previous to the attack is no bar to the use of the method, I may say that I attended a lady here, during an epidemic prevalence of childbed fever, who had a hemorrhage before delivery, in which she lost no less than ninety ounces of blood, which left her pale and feeble. She was shortly afterwards assailed with puerperal peritonitis of a violent form. In that case, I bled the lady to the extent of bringing down the violence of the pulse, and thereby saved a life, which I am confident must have otherwise been lost. In my essay, introductory to the volume before mentioned, containing the works of Gordon Hey, &c., I stated a case of peritoneal childbed fever, in which I bled the patient in all sixty-five ounces, and with the happiest success. I do not believe that the extension of the

area of phlogosis is in equal ratios in equal times; nor is the inflammation itself, whatever be its area, always moving to the culmination at a specific rate, like that of a variolous pustule. Hence the rule, not to bleed later than the twenty-fourth hour, cannot apply to all the cases; since, in some, the forty-eighth, seventy-second, or ninety-sixth hour finds the disorder less advanced than it usually is at the twenty-fourth hour.

Leeching.—Many persons who fear to use the lancet, have no hesitation to take away blood by leeches. It is doubtless true that, if the quantum of blood within the vessels be diminished, by whatsoever cause, the physiological conditions, relative to that diminution, must wait upon the abstraction. The question, then, as to the comparative usefulness of the two methods, whether by venesection or leeching, is one to be tried by a comparison of the advantages of them respectively. I cannot advise the Student to prefer the use of leeches, because I consider that mode of depletion less convenient, less exact, and therefore less manageable. In conducting the operation by venesection, there is no difficulty in stopping, in temporarily suspending, or in persevering, until, by the well-known signs in the pulse, or the physiognomical expression, respiration, hue, and statement of the patient, an opinion is accurately formed. Whereas, in leeching, we cannot stop instantly, nor judge accurately as to the quantity to be withdrawn. Some there are, who advocate the use of leeches on the ground that they take blood directly from the inflamed parts; but the younger Baudelocque has shown, in his beautiful prize essay on puerperal peritonitis, the fallacy of this notion. It is to the last degree improbable that the application of leeches to the abdomen can have any direct influence upon the inflamed capillaries of the mesenteric, mesocolic, or hepatic circulations.

I shall not presume to deny that leeching the abdomen may be a useful method for treating some forms or some stages of puerperal peritonitis. Nevertheless, I may aver that, within the bounds of my own experience, I have nothing favorable to say of it.

Cupping.—This is a method which is excessively inconvenient and inapposite to the cases. Women are, for the most part, attacked within seventy-two hours after delivery; and, at that period, the teguments of the belly are still too flaccid after their late distension to admit of the proper application of cupping glasses.

Emetics.—A singular instance is to be met with in our medical history, of the flattering delusion that follows the announcement of new remedies. At the session of the French Royal Society of Medicine, of September 6, 1782, a memoir was presented exhibiting the results of

M. Doulcet's new method of treating childbed fever. Dr. Doulcet was one day at the Hôtel Dieu, just as the first symptoms of a puerperal fever, then epidemic in that house, became manifest in a recently delivered woman. It commenced with vomiting. M. Doulcet, immediately seizing upon this indication, ordered fifteen grains of ipecacuanha, which the patient took in two doses. The doses were repeated on the following day. The medicine operated both as an emetic and as a cathartic, the evacuations being followed by a notable amelioration of all the symptoms. The dejections were promoted by the use of an oil-mixture with two grains of kermes, and the patient was saved.

Upon this good success, the chief midwife was instructed to watch for the very first symptoms in any future case, and immediately thereupon to proceed with the treatment as above, without giving time for the engorgement to become established. In fine, "Partout le succés fut le même; et en quatre mois, pendant lesquels l'épidémie regna avec fureur, près de deux cents femmes furent rendues à la vie;" "in all cases the success was similar, and, in the course of four months, during which the epidemic prevailed furiously, near two hundred women were restored to life; five or six only, who refused to swallow the emetic, fell victims to their obstinacy." The above is from the *Hist. de la Soc. Roy. de Méd.*, 1780 and 1781, p. 254.

All Europe was gladdened by Dr. Doulcet's wonderful good fortune, and he became at once famous, and but for his premature death shortly after this discovery, honors would have been heaped upon him. This method was tried everywhere, but everywhere in vain; and Dr. Fothergill, reporting upon the cases at the Royal Medical Society of London, hints that Doulcet's method might be by many regarded rather as preventive than curative. Was this a malicious hint?

Doulcet's method is gone clean out of fashion, and I do not suppose that physicians, as a body, anywhere rely, at the present day, upon fifteen grains of ipecac. and two grains of kermes as cures for childbed fever.

Prof. Moreau, while showing me some of his cases of childbed fever at the Maternité at Paris, in June, 1845, told me that, occasionally, they met with a form of the disorder that yielded favorably to Doulcet's method; but I gathered from his speech that such instances are "few and far between."

After what I have stated in the early part of this chapter, the Student will not expect me to advocate the use of emetics as a principal method. But there are, doubtless, not a few cases of the disease, which, beginning under circumstances of a saburral state, whether mucous or bilious, of the primæ viæ, might be greatly benefited by the happy

operation of an emetic so gentle and safe. One can hardly undergo its operation without bursting into copious perspiration during the emesis. A disk of incipient peritonitis might receive a salutary check, and acquire a tendency to diminish instead of continuing to expand its area, under the new nervous excitement and sanguine determination provoked by the impression of this drug made on the nervous mass, and by the emulging and succussive efforts and shocks of the act of vomiting. I therefore advise the Student not to overlook the properties of ipecacuanha, while casting about for the best means of checking an attack just begun. It would be both useless and cruel to subject the woman to such shocks after the abdominal peritoneum has become extensively invaded. The pain would be frightful.

Cathartics.—I know that certain persons condemn the use of cathartics in our cases. But seeing that, in general, women go into labor with overcharged bowels, and that, among the earliest mischiefs in the cases, is the tympanitis that speedily comes on, I cannot doubt of the propriety, and even necessity, of prescribing cathartics at an early stage, and of repeating them judiciously, in a manner to compel the inflated and blown-up intestines to contract upon and expel the gases that are extricated within their cylinders. It is certain that I have seen instances rendered intractable by angulation of the gut, an accident that I described at page 649; and it cannot be denied that an extreme degree of tympanic distension does materially contravene the purposes of the respiration, by lessening the stroke of the diaphragm, and thus leading to a partial state of cynanosis, or asphyxiation, which augments with the progressive meteoration of the belly.

There is not to be found, perhaps, a more efficacious cathartic for the commencement of the treatment than calomel in a full dose. Few individuals can be met with for whom ten or fifteen grains of that drug will fail to operate, and it is desirable to have neither failure nor long procrastination of the effect. Hence, I advise the Student to exhibit, very soon after he has performed his Gordonian venesection, at least ten, and if the patient is of a good constitution, fifteen grains of calomel, which it is useful to combine with a grain, or two grains, of opium. To insure a complete catharsis, the woman should take, within two hours after the dose, half an ounce of castor oil. After waiting four or five hours for the operation of the calomel, or even sooner than that, provided the meteorismus begins to appear, she should have an enema of castile soap and water, or one containing an ounce of common salt, dissolved in a pint of warm water.

By this method, the peristaltic muscles may be compelled to contract with much force and for so long a time, as to discharge not only

the residua of the digestions and saburral collections, but every cubic inch of those gases, whose accumulation and persistence within the intestinal tubes oppose the fortunate operation of all other remedies.

I believe that Chaussier's method is a good and safe one to be followed after these first operations, viz: The patient may take a tablespoonful of a mixture, composed of castor oil one ounce, with syrup of rhubarb two ounces, to be repeated *pro re natâ*. If such operations should be judiciously maintained, there will be increased abundance of secretions, whether gastric, bilious, or intestinal, the whole of which must be furnished by the capillary ramuli of the cœliac and the mesenteric arteries. But, as the visceral peritoneum derives its capillaries from those three great digestive trunks, it is easy to see that their circulation must be much relieved by copious mucous and bilious separations from the blood they convey in common with the vessels of the other coats of the bowel, and of the integral tissues of the other viscera of the belly. I shall not dwell further upon this subject here; but, before I lay it aside, I shall implore the Student not to confide the chances of his patient to such hopes alone. They are firm and good adjutors of the method—the method is venesection.

Stupings.—Stupings with very warm water, applied by means of flannels laid over the whole belly, lessen pain and incite to sweating. A patient will rarely fail to fall into perspiration soon after the application of warm stupes to the belly. But, as this process of stuping is troublesome, and as the binder cannot be conveniently used while it goes on, I prefer to get at the same result by means of very light, but hot cataplasms, such as thin mush of corn-meal or lint-seeds, mixed with hops, stewed to a proper pultaceous consistence; or chamomile; or tansy with fresh mint, combined with the emollient lint-seed or with rasped bark of the slippery-elm. This adjuvant should be resorted to very early, and persisted in until the resolution of the inflammation becomes sure.

Blisters.—I regard the woman under childbed fever as supporting a burden of irritations too great too be easily borne, and I will not add another so intolerable as this. I grant that women have recovered who were blistered, but do not now remember to have seen more than one such. The physician ought to repeat his diagnostications with every renewed visit to the bedside: to blister the belly is to put a veil betwixt his senses and the operations going on within the cavity; he cannot discriminate between the pain and soreness of the remedy and those of the disease. I, therefore, do not recommend blisters in childbed fever.

Oil of Turpentine.—A Dr. Brenan, of Dublin, awakened, about

forty years ago, the highest hopes of the profession, as to a remedy for childbed fever, by the successful treatment of a few cases with doses of oil of turpentine, and the employment of it as an embrocation for the abdominal region. I shall not discuss the question as to the efficacy of oil of turpentine, nor the stage of the disorder in which it is most applicable. Dr. Meissner has given a learned *resumé* of the authorities, to which the curious Student may refer.

Opium.—These cases can hardly be well conducted without the aid of opium as a helper to the cure. I have already signified my approbation of the combination of it with calomel, and doubtless, when our chief evacuating methods shall have been once carried into effect, we shall be compelled, or rather we shall anxiously wish, to obtund the nervous impressionability by a moderate induction of the opium anæsthesia.

The drug may be exhibited in the form of Dover's powders, repeated in doses of three or five grains every hour or two, until the tranquillizing or soporific force of the drug is made manifest; but it will often be found both more useful and more convenient to exhibit the drug as laudanum or black drop, with small starch or mucilaginous enemata, especially when there are great pain and irritation within the pelvis or in the womb. Some persons have relied upon the exhibition of opium in very large doses, frequently repeated, as in delirium tremens. I do not believe such a method will be crowned with a general success.

Mercury.—It is thought that mercurials possess an aplastic power, as it is called; that is to say, they determine the evolution in lessening ratios of fibrine or plastic lymph in the blood. I do not regard this as more than an hypothesis; and it cannot be doubted that where stomatitis or ptyalism is produced by the drug, the evolution of fibrine is considerably increased instead of being lessened. It was formerly deemed desirable, at least by many, to procure an early salivation as a cure for the peritonitis. Hence, women have swallowed from three to nine hundred grains of calomel in the course of a few days, and many have been blistered upon the belly, with a view to get an exposed corpus-mucosum, on which with greater certainty of success to apply the salivating ointment of quicksilver. This appears to me to be a barbarity, and a useless one too, since I conceive that the woman who should recover with it would more readily recover without it.

Antimonials.—A sick person, who is brought under the condign influence of tartar emetic in fractional doses, will be found to have the systole somewhat lessened in force and frequency; but great caution is demanded in the just handling of the drug. If given in excess, it

causes great distress from the nausea or from vomiting, and is likely to excite unfavorable relaxation of the bowels; nevertheless, in all inflammatory diseases, the preparations of antimony, judiciously administered, tend to the subduction of the vascular and nervous reaction of the economy against the topical lesions. I cannot advise the Student to trust his childbed fever cases to the sole power of antimonials, as therapeutical agents; but, after the evacuations have been duly premised, these medicines are, in my opinion, among the most indispensable of remedies.

My own clinical experience teaches me that, in using them, it is desirable to use them just *citra-nauseam*. If they provoke to positive nausea and disposition to vomit, they only add to the load of irritations already sufficiently intolerable. Hence, I am accustomed to use very small doses, as thirtieths or fiftieths of a grain, either dissolved in pure water, or in water along with a minute portion of morphia.

Such a prescription would prove unsuitable in those cases, not unfrequent ones, in which there is strong disposition to diarrhœa. Doulcet employed the kermes after his ipecacuanha doses, and the celebrated Chaussier was accustomed to rely much upon the same article after evacuations by the lancet and by cathartics.

Enemata.—It appears to me that enemata constitute a chief, and, indeed, indispensable part of the *armamentarium medicum* in these cases. One of the most striking phenomena of childbed fever is the meteoration, or tympanitis, that attends upon almost every case. It could not be expected that the serous coat of the alimentary canal should be greatly and extensively inflamed without producing its inevitable influence upon the muscular apparatus of the bowels, which refuse to contract beneath the sort of shirt of Nessus that wraps them in the fiery covering of an inflamed peritoneum.

I do not wish to discuss the question whether the gases that inflate and distend the bowels are secreted or extricated. Be the source of those gases what it may, their presence within the tube is a disastrous accident in many of the cases. I have observed women, attacked at four A. M., to be much more distended at ten in the forenoon than they were in the last days of the pregnancy; and the whole of this enormous distension arose from the presence of gases within the stomach and the great and small intestines. The belly, in some individuals, has been swollen as if it might burst, being perfectly elastic under pressure, and sonorous as a drum. Let the Student reflect upon the aggravation of distress that must exist under such a condition, and let him consider how vastly the superficies of inflammation is expanded by the expansion of the superficies of the bowel, and how

great an advantage would be likely to result from any method by which the diameter of the intestine could be reduced to and maintained in its normal measures!

As to the colon, it is at least six feet in length. It occupies the cavity of the belly in common with the small intestines and the viscera. Should the whole colon—six feet in length—become larger than a stout man's arm, it is clear that its turns or convolutions can no longer be effected in curves; but the returns must be effected by acute angles, or angulation of the gut; such a distended tube, bent on itself at an acute angle—I have seen three such acute angular returns in one dead body—is equal in its obstructing power to a ligation of the bowel; nothing, not even gases, can pass beyond the point of angulation, and thus the miserable woman has a strictured gut to contend with in addition to all the other causes of destruction.

These remarks ought to show the Student, that, when alarmed by discovering the first small area of phlogosis, he should at once ponder upon the possible effects of the sure coming intestinal inflation. Cathartics, then, would be deemed indispensable, and the practice of Chaussier and others, who so solicitously attended to the due excitement of the muscular apparatus of the bowels, admirable. But we cannot venture to administer cathartics too frequently. Hence the value of the method by enemata. Clysters of aperient mixtures serve at least to compel the colon to discharge the gases that so dangerously distend it, and they should be repeated and reiterated according to the exigencies of the case. Enemata, composed of infusion of lint-seed mixed with castor oil, or of solutions of Spanish soap, or of mucilaginous fluids containing table-salt, or jalap, or extract of senna, procure the greatest relief, and add to the chances of cure. They fatigue the patient but little, and do not exhaust her strength. Sometimes enemata composed of an ounce of castor oil with half an ounce of oil of turpentine, made into an emulsion with mucilage or white of eggs and water, may procure the expulsion of gases that will not be brought away by any other compound.

For the childbed fever patient one invariable command should be insisted upon; I mean to say she should not leave the bed, nor sit up for the convenience of the evacuations; even the pillows should not be very high ones, for the patient is safest in a low recumbent position.

In the beginning, let the diet be absolute for twenty-four or thirty-six hours, and after that a light barley-water little sweetened, or rice water, or gum arabic water, with a cup of tea, or milk and water, equal parts of each with a morsel of bread.

I regard oatmeal gruel as detestable for such patients, as it invari-

ably, even in women who are not attacked with any disorder, produces flatulency. So sure is this flatulency to arise, under a diet of oatmeal gruel, that I think I can venture to say few women use it in the first days of the lying-in without suffering more or less from the meteoration of the abdomen. In my *Letters on Women, &c.*, p. 595, I have quoted a passage from Rainald, on this very subject. The objection to "*ote mele cawdels*," which was so strongly urged in 1540, has been valid against that miserable but universal feed for the lying-in woman ever since that date. I protest against it. Gruel made of Indian meal, or fine South Carolina hominy, is totally free from such objections. A woman who is content to live for a few days on hominy gruel, with a portion of tea and toast, is unlikely to suffer from flatulent distension of the abdomen. Good cocoa, with milk and stale bread or toast, is a very proper and safe diet. As the disease advances, whey, chicken-water, beef-tea—but it is unnecessary for me to enumerate articles of diet for the advanced cases.

The Student will, I hope, learn to regard childbed fever as not a fever, but inflammation, and as inflammation commencing with small areas of phologsis in tissues prepared and prone to admit of frightfully rapid and destructive expansion of those areas. I do not mean to deny that, under the influence of extraordinary virulence of the epidemic cause, and in women predisposed by poverty, ill diet, terror, etc., the peritoneum may at once inflame in large patches. Indeed, some of the cases noticed by Dr. Collins, and some that I have witnessed, have led me to conclude that the phlogosis does rarely commence by a sudden erythema of the peritoneum, which, immediately passing into inflammation, causes the nervous mass to succumb at once under such multitudinous and insupportable irritations. So that the woman, if examined now, is well, and in an hour is found to be irrecoverably ill. These terrible cases admit of no treatment. Attempts to cure them seem only to aggravate the pains of the dying, and discredit both the physician and his art.

Nevertheless, childbed fever, even epidemic childbed fever, ought to be a tractable malady; and I am persuaded will be found so, provided the medical attendant should exercise a due vigilance at first, and afterwards a real intelligence and an undaunted resolution to carry out in practice the measures deduced from the pathology of the case.

I never doubt of Gordon's correctness on the subject of early treatment; but that early treatment must depend upon the vigilance of the physician himself. The vulgar notions on physic are not to be expected to put people upon guard against the earliest hardly dis-

cernible rudiments of a malady which requires only to be set on foot, to run a race so fleet as to defy all attempts to overtake and arrest it.

If Gordon is right in declaring that we shall save the patient if we begin early, let us resolve to begin earlier; let us take measures to insure the application of the *method*—there is but one method—as soon as the signal is given. If we can cure within the first eight hours, how much more may we confide in our powers if we begin with the very beginning! Wherefore, I let the Student watch—let him watch with a panoptical vigilance; and if the chill, the ague, and the pain—if the diagnostic say or even hint the area is formed, let him abstract such a quantity of blood as shall compel the heart to beat just as softly, and inject the vessels just as moderately as, in his judgment, he may wish or will it to do. He can limit in this manner the area of inflammation, and at the same time impress upon the inflamed tissue a tendency to recover by resolution, which is safety; and restrain it from terminating in effusion, which is death.

The cases of childbed fever, in which it assumes the form of metritis or uterine phlebitis, are different in character from those of the peritonitic form. The pain in the metritic cases, though severe, is more dull. It is confined within the pelvis, and does not develop the tympanitic disorder as in the other examples. In its earlier stages there is often exquisite pain, produced by spasmodic contractions of the muscular apparatus of the womb, in what is called after-pain. This is the reason why, in the commencement, we are sometimes greatly embarrassed by the question whether the attack be merely spasmodic, or whether it be metritic. I advise the Student to endeavor to settle this important doubt, for if it be after-pain, or rheumatism of the womb, it may safely be treated by the use of narcotics; whereas death must follow a mistaken diagnosis of after-pain or rheumatismus uteri, when, in fact, a pure phlogosis of the womb has begun its career. Let him, then, not only estimate the discharges, the place, the pain, the soreness, the breathing, the temperature, the pulse, and the physiognomical character at the time, but let him make the Touch, in order to have a clearer report from the organ itself—so that, if it be indeed a phlegmasia, he may bleed at once, and in a way effectually to fulfil the command, *Jugulare febrim.*

Uterine phlebitis rarely occurs alone, or uncomplicated with metroperitonitis. The inflamed state of a vein gives little or even no pain at first. It becomes a painful disorder only when the phlogosis has sunk below the true bloodvessel, or endangium, into the basement tissue on which the membrana vasorum rests. When thus inflammation has passed through the fibrous coat, and has attacked the cellular

sheath in which the vessel is inclosed, then the pain becomes considerable, for it is a phlegmonous inflammation. Hence, uterine phlebitis is the most insidious of disorders, and has often attained to an incurable stage, before even the constitution of the woman takes the alarm, and begins to sink under it.

Whenever it shall have acquired a certain degree of intensity, and also begins to be pyogenic, and when corpuscles of pus, washed away from the veins by the torrent of the circulation, are transported to the heart, by which they are injected into all parts of the systemic circulation, this purulent infection of the blood manifests itself, not solely in the rate of the heart's action, which it greatly hurries, but especially by its influence upon the psychical state of the woman.

In pyæmia, we have the strangest hysteroid phenomena. Pus-corpuscles in the blood causes pyæmic intoxication. Indeed, one is apt to be grossly deceived; to believe he has in hand a case of hysteria or insanity; but, far from that, he is dealing with a mortal pyæmia, a state in which the blood is charged with pus-corpuscles that exercise an influence upon the nervous and psychical forces of the woman, closely resembling the effects of arsenic in poisoning doses.

I will not say that pyæmia from phlebitis shall kill; yet, certainly, I believe we cannot meet with diseases more unmanageable. We ought to arrest the malady *in inceptû*, by a Gordonian venesection; if we fail in success, then we must expect the death of the victim, or contend with a long and terrible malady, during which the economy painfully and slowly eliminates the purulent corpuscles, and at last restores the healthy crasis of the vitiated blood.

Pus, thus generated, and thus transported by the circulation, is collected again in the form of cold abscess, which may declare itself, in the course of a single night, or lay the foundation for the most painful and slow imposthume.

I have seen a patient under pyæmia go to sleep at night with her hand in perfect order, and awake in the morning with the whole space between the left thumb and forefinger filled and plump with the pus transported and deposited there in a few hours.

A lady here under my care was seven months ill, requiring daily visits. During that time, she deposited pus in eleven distinct abscesses, and did not recover her health or become wholly free from the pyæmia until a short time before she recovered her health.

In the conduct of our cases of childbed fever, we should endeavor to make a correct prognosis. It is always unfavorable if we commence the treatment more than twelve hours after the beginning. A violent assault, evincing its violence by great pain, ague, early meteorismus and tympany, rapid expansion of the seat of pain, pulse of 140

per minute, uneasy state of the stomach, altered physiognomical expression, great and early debility, and a presentiment of the patient that she will not recover, is highly unfavorable. On the contrary, a slow expansion of the seats of pain, pulse softened and diminished in frequency, the failure of the meteoration, warm and abundant diaphoresis, fair toleration of the venesection and medicines, a cheerful mind, lessening temperature, absence of rigors, moderate thirst, no eructations—these, proceeding from better to better, shall augur the success of our treatment.

But, inasmuch as I have already drawn out this chapter to a length far greater than I had intended, and as I have in a special treatise put forth my thought upon the nature, seats, causes, signs, and treatment of the malady, I shall now bring it to a close with the earnest recommendation to any Student who may honor it with a perusal, to purchase and carefully to read at least the volume on Puerperal Fever, published here by Messrs. Barrington and Haswell, and which contains not only the precious work of Gordon, but the impayable treatises of Hey the younger, Drs. Armstrong and Robert Lee. Let him read at least these, if not mine, and as many others as he can lay his hand upon.

Before closing this chapter, I beg leave to refer again to what I said in regard to the action of the epidemic cause upon the nervous mass of animals. The more I reflect upon the nature of animals, the more do I become convinced that all impressions from without or from within are made upon that part of our animal substance that is called nervous substance. I have been very much surprised by an expression of Prochaska, at p. 387, *Prin. of Phys.*, published by the Sydenham Soc., who says "that a nervous system is not present in all animals," &c. &c. It is hardly true that many insects have not cerebra; and though infusory animals are destitute of brain and nerves, it does not follow that they are deprived of, or could exist without a nervous substance. The first of physiologists, Oken, has clearly shown that, even in creatures who are destitute of brain or nervous tractus, there is distributed everywhere in their material, a portion of nerve substance, that serves as the animating principle of all the rest of their material or substance. It seems to me that no person could follow him in his exposition of the psychical homologies of animals, rising from the lowest grade to the highest eminence in the zoological series, without becoming convinced that, even in creatures unprovided with what might be called a nervous system, there is for them abundant provision of nervous mass which, in my view of it, is the very essence of animality. Changes effected in the crasis or perfection of this nervous mass may well account for the symptoms developed by *epidemic cause.*